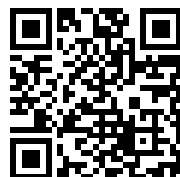

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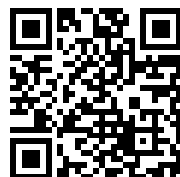
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TRANSACTIONS

OF THE



AMERICAN
PHILOLOGICAL ASSOCIATION.

1876.

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PROCEEDINGS:—Eighth Annual Session, New York, 1876.

TRANSACTIONS
OF THE
AMERICAN PHILOLOGICAL ASSOCIATION.
1876.

I.—*On εἰ with the Future Indicative and ἰάν with the Subjunctive in the Tragic Poets.*

BY BASIL L. GILDERSLEEVE,

PROFESSOR OF GREEK IN THE JOHNS HOPKINS UNIVERSITY.

ON looking over the proceedings of this Association for the last few years, I have noticed that the conditional sentence in Greek has had at least its full share of attention ; and I should hesitate to introduce the subject at this meeting, especially in the absence of the distinguished scholar whose treatment of this important class of sentences has found so wide an acceptance in this country, if the paper which I had prepared in the hope of meeting Professor Goodwin here were not of a statistical rather than of a theoretical character.

At the same time, a general theoretical preface is necessary in order to make my results intelligible ; and I will endeavor to state, as simply as possible, the view of the conditional sentence which I have found to be serviceable in the work of practical instruction.

In common with most grammarians, I divide the conditional sentence into four classes, for which I have been in the habit of using the designations "Logical," "Anticipatory," "Ideal," "Unreal." If nothing more can be said in behalf of this nomenclature than that it saves time, something at least has

been said; and I am glad to learn that a part of this nomenclature, as applied to the Latin language, has found favor among teachers. Logical, Ideal, and Unreal conditions occur in Latin also. The Anticipatory is peculiar to the Greek.

1. 1. The *Logical* condition states the elements in question. It is used of that which can be brought to the standard of fact; but that standard may be for or against the truth of the postulate. All that the Logical condition asserts is the inexorable connection of the two members of the sentence. Aeschines gives us a good type (3, 188): *εἰ τοῦτ' ἔχει καλῶς, ἐκείνο αἰσχροῦς, εἰ ἐκεῖνοι κατ' ἀξίαν ἐτιμήθησαν, οὗτος ἀνάξιος ὦν στεφανοῦται.*

It is the favorite form of condition in argument. So the chorus in Thesmoph. 789 makes a home thrust when it says: *εἰ κακόν ἐσμεν, τί γαμεῖθ' ἡμᾶς, εἴπερ ἀληθῶς κακόν ἐσμεν;*

It is the fairest form, and so Demosthenes as he introduces it (18, 10) says: *θεάσασθε ὡς ἀπλᾶ καὶ δίκαια λέγω.*

I call it the Logical condition on account of this argumentative use, and compare it with the indicative question and corresponding indicative answer.

2. The Logical condition, like every other form of the conditional sentence, is particular or generic according to the character of the apodosis. Hence when it has its apodosis in the present, it has a double meaning, which adapts it admirably to personal argument. So especially when the form *εἰ τις* is used, which may point either to a definite or to an indefinite person, the Logical condition is a two-edged sword, often wielded in the keen encounter of Attic wit. But as the *ἐάν* conditional with a present indicative apodosis is regularly generic, it is not without reason that this form should be preferred, when distinctly generic action is to be expressed. Just as the conative element is not so distinct in the present as in the imperfect, simply because the present has the double function of a present of continuance and a present of attainment, so the *ἐάν* conditional is more distinctly the generic conditional. But take such an example as AESCHIN. 3, 196: *εἰ τις ἐν δημοκρατίᾳ τετιμημένος πολμᾷ βοηθεῖν τοῖς παράνομα γράφουσι, καταλύει τὴν πολιτείαν ὑφ' ἧς τετίμηναι;* and I am unable to see why such a gnomic sentence

should not be generic.* And when we turn from prose to poetry, we find the Logical condition freely employed in generic as well as in particular relations, doubtless because poetry loves the more concrete form. But here again we must distinguish; for in the epic the subjunctive is the regular construction in generic relations, while we find the Logical condition more frequently in dramatic poetry than in prose. Note also that proverbs delight in logical forms.

II. In the *Anticipatory* conditional sentence the action of the protasis is anticipated. For this form of the condition we want a word that will harmonize present and future. Anticipation is not expectation, though it is loosely used for expectation and may be stretched to cover it. Anticipation treats the future as if it were present; and, as we find a useful parallel for the Logical condition in the simple indicative question, so we can best illustrate the Anticipatory conditional by the imperative, as Curtius and others have done.

The Anticipatory conditional was divided thirty years ago by Bäumlein (notably in his *Griechische Modi*, s. 219 and 221) into the particular and the generic, according to the character of the apodosis; and I have found this division of no little practical service in my experience of twenty years. Professor Goodwin, as is well known to you, came to the same result independently and made a more extensive application of the analysis—too extensive, as he himself has admitted with a frankness which does him all honor.†

The Anticipatory conditional, then, as I will permit myself to call it, is the more common form of the generic conditional and the usual form of the toneless future conditional—of the postulated future. How it differs from *εἰ* with the future indicative or logical future, it is the object of this paper to illustrate rather than to prove.

It may be worthy of especial note that the Anticipatory condition is invariably used in laws, and it may also be called the Legal condition.

* Examples of the generic logical conditional might be multiplied indefinitely: see XEN. Oec. 11, 24; PLAT. Legg. 9, 865, A, B; DEM. 23, 54 (an instructive passage).

† See Transactions of American Philological Association for 1874.

In the early language and occasionally in Attic poetry, rarely in Attic prose, *εἰ* with the subjunctive is found in very much the same sense as *ἐάν* with the subjunctive. I am not disposed to refine on the difference; but I think that I have noticed that the tone approaches *εἰ* with the future indicative. This form, *εἰ* with the subjunctive, seems to be the older, and hence, when there is a transfer to *oratio obliqua*, the conditional appears as *εἰ* with the optative, and so the apparently exceptional disappearance of an *oratio recta* *ἄν* is satisfactorily accounted for.

In these transfers of the Anticipatory conditional to the past, we have again the division into particular and generic—for the so-called optative of indefinite frequency is nothing but the *oratio obliqua* of the subjunctive; for even when the *oratio obliqua* is not formally expressed, it lies in the notion of will, inclination, habit, which is involved in rule of action.

As the Ideal and Unreal conditionals do not come within the scope of this paper, I may say briefly that

III. The *Ideal* condition (*εἰ* with the optative—optative with *ἄν*) is the condition of the fancy. There is often an element of wish for or against, of hope, of fear. It is the great condition of illustration, and is often used in comparisons, where those untrained in Greek modes of conception would expect the Unreal condition.

IV. In the *Unreal* condition (*εἰ* with indicative—indicative with *ἄν*), the protasis is against reality. It is the other side of the Logical, and, like the Logical, is often used in argument. The formula of the Unreal condition is, in my judgment, too narrow, and the opposition should be represented as opposition to continuance, attainment, and completion, and not to past and present simply.

But it is time to return to the point under special consideration, the relation of the Logical Future Condition and the Anticipatory.

It is a matter of every-day grammar that *ἐάν* with the subjunctive is the common form of the future condition in Greek, and the parallel is often drawn between *ἐάν* with the

present subjunctive and *εἰ* with the future indicative, *εἰάν* with the aorist subjunctive and *εἰ* with the future perfect indicative; and it would be well, in my opinion, if teachers were to limit themselves to this statement, and leave out the more or less foreign elements of probability and practicability. It is very true that the anticipatory conditional has to do with practical matters chiefly, but so has the parallel imperative, and yet the imperative is not bound by probability or practicability, and when Menelaus says to Orestes (EUR. Or. 1593):

ἀλλ' οὐτι χαίρων, ἦν γε μὴ φύγῃς πτεροῖς,

"But to thy sorrow, an thou do not 'scape by wings,"

there is a sheer contradiction of the course of nature. So EUR. Phoen. 1216: ἦν μὴ γε φεύγων ἐκφύγῃς πρὸς αἰθέρα.

The fact then is patent enough to every one who will be at the pains to count, that for model Greek prose *εἰάν* with the subjunctive is preferred to *εἰ* with the future indicative. The reason of this seems to be to a considerable extent the greater temporal exactness, the same greater temporal exactness which has wholly displaced the future indicative with the temporal particles, the same greater temporal exactness which has given so wide a sweep to the optative with *ἄν* as a sharper form of the future.

In future relations, *εἰ* with the future indicative may be dissected into *εἰάν* with the present subjunctive and *εἰάν* with the aorist subjunctive; and hence, whenever it is important to distinguish continued from concentrated action, whenever it is important to distinguish overlapping from priority, *εἰάν* with the subjunctive is preferred, not only in general sentences but in particular sentences; and it is with particular sentences that we have to deal in this essay.

Now the neglect of this distinction in *εἰ* with the future indicative shows a certain coldness, a certain indifference; and this added to the general rigor of the logical condition, which faces fact in all its grimness, gives a stern, minatory, prophetic tone to the future indicative, which commentators and grammarians have noticed, but noticed only in passing, and noticed without attempting to account for it. Especially familiar is the combination *εἰ μὴ τις*, in which *τις* is often a

covert second person. In the practical handling of the language in Greek composition, I have made it a rule to exclude *εἰ* with the future indicative except in harsh threats, solemn warnings, in passages where *δεῖ* or *μέλλω* would be appropriate (so-called modal use of the future), and with verbs and phrases of emotion.

In striking contrast to the comparative infrequency of the combination *εἰ* with the future indicative in prose (and I may add in epic poetry), is the frequent use of it in the tragic poets; and having had the curiosity to examine the comparative usage of Aeschylus, Sophocles, and Euripides in this regard, I have thought that the results might be of some interest to the Hellenists of this Association. In preparing my statistics, I have excluded all clearly generic sentences; but even if these should be included, the result would not be materially affected; and, though I at one time intended to meet any possible objection based on the mechanical *metri causa*, I found subsequently that the usage of Aristophanes completely disposed of any such superficial cavil. In spite of fashionable sneers at oversubtlety of distinctions and overrefinement of syntax, I am a firm believer in the organic unity of artistic character; and I should expect to find a specific difference under the common type in every fibre of the living organisms of Greek literature. Surely if the mechanical structure of the verse of Vergil and the verse of Ovid reflects the diverse ideals of the two poets, we may expect to see in the varying employment of the sterner forms of expression a manifestation of the character of the three great tragic poets. I grant, in advance, that the induction is not so wide as I could desire in the case of Aeschylus, who seems not to delight in conditional expressions so much as Sophocles, and far less than the lawyer-like Euripides; but even this is a significant fact and may deserve a closer notice.

As I have already intimated, the three tragic poets are faithful to the same general type. They all deal largely in the future conditional, but in a decreasing ratio—always far exceeding the normal usage of prose, but still differing in their individual conception.

In brief, of the future conditions in Aeschylus over seventy-three per cent. have the future indicative, in Sophocles over fifty-four per cent. (and if we leave out the Oedipus Coloneus and Philoctetes, as I think we may, as much as sixty-two and one-half per cent.), while Euripides, approaching, as he does, more nearly to the language of prose, puts the majority of his future conditions in the *έάν* form, so that only about forty-three per cent. have *ει* with the future indicative.

Of course, I cannot undertake to discuss the hundreds of conditional sentences which I have examined in this way, nor even to comment on the more striking forms. A condensed tabular exhibit has been prepared to accompany this paper.

I would only add that, so far as I have observed, *ει* with the future indicative and *έάν* with the subjunctive are seldom used in antithesis. Where such antitheses occur they are in the main confirmatory of the difference of tone already recognized. So ARISTOPH. Nub. 586. 591, in which *ει στρατηγήσει* Κλέων presents the threatening, unfavorable alternative, *ήν* Κλέωνα φημύσσητε the favorable view. So in XEN. Cyr. 4, 1, 15 the favorable alternative is introduced by *ήν μέν*, the unfavorable by *ει δέ* with future indicative. In DEM. 27, 20. 21. 22, *έάν μέν* φῆ—*ει δ'* αὐ φήσει—*ει δ'* αὐ έρεῖ, we may recognize an increasing hardness of tone, although that is not to be insisted on. A clear case is the one to which Professor Price, of the University of Virginia, has called my attention in DEM. 8, 17: *άν μέν* τοίνυν ἦ τὸ συνεστηκὸς τοῦτο σπράτευμα (favorable), *ει δ'* ἅπαξ διαλυθήσεται (unfavorable); and yet others occur in ISOCR. 6, 107; 12, 237; 15, 130; LYS. 27, 7. In ISOCR. 17, 9 three courses are open, the worst being put in *ει* with future optative (for future indicative), the other two in optative (for subjunctive), the passage being in *oratio obliqua*. In HEROD. 6, 11, *ήν μέν* βοόλησθε—*ει δέ* διαχρήσεσθε, the same thing is found; but in 1, 71, *ει* νικήσεις—*ήν* νικηθῇς, both alternatives are unfavorable, and in 3, 36, 3, *ει μέν* μεταμελήσει—*ήν δέ* μὴ μεταμέλῃται, the usual practice seems to be reversed (as in HOM. II. 1, 135. 137).

A striking example, which lies, however, beyond our present sphere of observation, is found in THEOCRIT. 7, 107. 109, in

which deliverance is besought for Pan in one case ($\eta\gamma\mu\epsilon\nu\tau\alpha\upsilon\tau'\epsilon\rho\omicron\eta\varsigma$), all manner of evil invoked in the other ($\epsilon\iota\delta'\alpha\lambda\lambda\omega\varsigma\nu\epsilon\upsilon\sigma\epsilon\iota\varsigma$).

But while the antithesis of $\epsilon\alpha\nu\mu\epsilon\nu$ with the subjunctive and $\epsilon\iota\delta\epsilon$ with the future indicative is not common, the elliptical use of $\epsilon\iota\delta\epsilon\mu\eta$ following $\epsilon\alpha\nu\mu\epsilon\nu$ is one of the stock observations of the grammars. This want of correspondence in the antithesis may be accounted for by the phraseological use of $\epsilon\iota\delta\epsilon\mu\eta$, 'otherwise,' *sin minus*, as one of the numerous unconscious ellipses of which every language is full; but it is worth noting that $\epsilon\iota\delta\epsilon\mu\eta$ gives not only the negative but, as a rule, the unfavorable alternative also (e. g. EUR. Iph. Aul. 916. 917; Med. 241. 248), and this minatory and monitory character would suggest the distinct ellipsis of the future indicative, unless indeed $\epsilon\iota$ with the subjunctive may be considered an equivalent.

A striking contrast to the usage of the tragic poets is presented by Aristophanes. In his future conditionals, little more than twenty per cent. take the form $\epsilon\iota$ with the future indicative; and although the count was a rough one, owing to want of time for a careful exploration, the result is sufficiently accurate to establish the peculiar character of this form. Of sixty-seven such conditionals that I have examined, fifty-three are minatory or unfavorable; and it is no stretch of fancy to see in the others a certain mock-seriousness, which is one of the charms of Aristophanes.

I had hoped to find time for the collection of statistics as to the usage of Attic prose-writers; but as the fact of the preponderance of the $\epsilon\alpha\nu$ conditional is sufficiently familiar, I have been content to limit myself for the present to two or three specimens.

And first of Thucydides. As every one knows, the usage of this remarkable writer is highly individual and by no means characteristic of normal Greek prose. No wonder, however, that in the greatest tragedy of Greek history, he should have approached the standard of tragic expression and exhibited proportions which are closely those of Euripides.

The speeches in Thucydides show an unusually large proportion of Logical future conditionals. Of his future condi-

tionals nearly forty-five per cent. take the logical form, and of these logical futures only five out of fifty-two do not distinctly indicate an unfavorable hypothesis, or, in other words, are not distinctly "monitory and minatory." This minatory character makes the natural tonelessness of the other form appear favorable by comparison, and in fact we find that in the majority of *εἰάν* conditionals the more favorable side is assumed. Comp. 5, 111, 2: *εἰ μὴ σωφρονέστερον γνώσεσθε* with § 4: *ἦν εὖ βουλευέσθε*; and the tone of the angry speeches of the Corcyraeans and Corinthians (1, 32-43) and of the warning address of Archidamus (1, 80 foll.) with the *paraeneses* of Hermocrates (6, 33 foll.) and of Nicias (6, 61).

In twelve orations of Lysias—the first thirteen, omitting the *Epitaphios*—I find twenty-one sentences with *εἰ* and future indicative against twenty-four with *εἰάν*. To the latter may be added, however, seven optatives in *oratio obliqua*, which are clearly transferred from *εἰάν* with the subjunctive. Here too, as in Thucydides, we note a much larger proportion of the sterner forms than is usual in prose, not a much larger proportion than might have been expected from the incisive genius of Lysias. All the examples may be referred to the classes mentioned. The conditions are either minatory and monitory, depend on verbs or phrases of emotion, or involve a modal use of the future. (See 3, 37. 43; 6, 13; 7, 41; 8, 1; 10, 10. 22 (*bis*); 12, 11. 29. 35. 70. 83. 85. 88. 90 (*bis*); 13, 15 (*bis*); 13, 93. 94.)

In Isocrates, I have selected three discourses—the *Philippus*, the *Archidamus*, and the *De Permutatione*. In the courtly *Philippus* I have noted thirty-two particular anticipatory conditionals and only two in *εἰ* with the future indicative—one referring to Isocrates himself (§ 82), the other to the Greeks generally (§ 121)—one unfavorable, the other monitory. In the martial *Archidamus* there are seven logical futures against twelve anticipatory conditions, and all of them fall under one or other of the categories mentioned (§§ 2. 10. 13. 28. 40. 56. 107)—the last one (§ 107) giving the unfavorable antithesis of a condition with *ἦν*. In the *De Permutatione*, I have counted some twenty-four anticipatory conditionals and only

four logical futures (§§ 130. 152. 243. 272). In § 130 we have another antithesis between the two forms—the less favorable preceding with *εἰ μὲν*, the more favorable following with *ἤν δέ*.

In Demosthenes *De Corona* *εἰ* with future indicative occurs only six times (§§ 7. 63. 114. 160. 205. 207), every time with stern, impressive tone; and in the oration against Meidias about twenty six per cent. of the future conditionals assume this form, all twelve examples noted (§§ 57. 100. 106. 109. 118. 131. 160. 204. 206. 212. 221. 222) being minatory or minatory, emotional or semi-causal.

In the speeches of Xenophon's *Anabasis* the proportion seems to be about the same as in Aristophanes. Worthy of note here also is the largely minatory character of *εἰ* with future indicative. Of thirty such conditionals that I have examined, all but four are distinctly minatory and the others are to be referred to the category of *δεῖ* or *μέλλω*.

In comparing the frequency of the *εἰάν* conditional with the frequency of the future logical, it may be well to take into consideration the abridged conditional sentence or the participial expressions of conditional relations. All conditional participles which are connected with future tenses must be resolved into *εἰάν* with subjunctive: *λαμβάνων* = *εἰάν λαμβάνω* *-ης -η*, *λάβων* = *εἰάν λάβω* *-ης -η*. There is no participial expression for the stern future indicative. The tone is lost in the emphasis of the temporal relation.

I pass now to an enumeration of the forms in the Tragic Poets. In the first column are given the occurrences of *εἰ* with the future indicative, in the second those of *εἰάν* with the subjunctive.

AESCHYLUS.*

PROMETHEUS VINCTUS.

- | | |
|---|--|
| (1). 311 <i>εἰ... ῥίψει</i> (minatory). | (1). 328 <i>πειράσσομαι, εἰάν δύνωμαι</i> (tentative, i. e. semi-interrogative). |
| | (2). 1014 <i>εἰάν μὴ πεισθῇς</i> . |

SEPTEM CONTRA THEBAS.

- | | |
|--|---|
| (1). 196 <i>εἰ μὴ τις ἀκοίσεται</i> (minatory). | (1). 242 <i>εἰάν πίθησθε</i> (may be considered generic). |
| (2). 618 <i>εἰ καρπὸς ἔσται</i> , 'is to be,' (certainty). | (2). 1027 <i>ἤν μὴ τις... θέλῃ</i> . |

* The text followed is that of Dindorf.

PERSÆ.

- (1). 357 εἰ . . . ἰζεται κνέφας (certainty). (1). 529 εἰάν περ μόλῃ.
(2). 369 εἰ . . . φευξοίαθ' Ἑλλήνες—fut.
opt. in *orat. obl.* for fut. ind. (minatory).

SUPPLICES.

- (1). 461 εἰ μὴ . . . ὑποστήσει (minatory).
(2). 472 εἰ . . . μὴ . . . ἐκπράξω (minatory).
(3). 474 εἰ . . . ἦξω (minatory).
(4). 511 εἰ (supply ἐκδώσεις).
(5). 902 εἰ μὴ τις . . . εἰσιν (= fut.) (minatory).
(6). 924 εἰ τις . . . μὴ ῥησθήσεται (minatory).

AGAMEMNON.

- (1). 207 εἰ δαίξω, 'am to,' 'must.' (1). 1424 εἰάν . . . κραίνη θεός.
(2). 1059 εἰ τι δράσεις, 'intend to,' (2). 1667 εἰάν δαίμων . . . ἀπενθύνῃ.
(minatory).
(3). 1249 εἰπερ ἔσται γ', 'is to be.'
(4). 1338 εἰ ἀποτίσει, 'is to.'

CHOËPHORI.

- (1). 182 εἰ . . . μήποτε ψαίσει, 'is to.'
(2). 273 εἰ μὴ μέτεμι, 'am not to.'
(3). 571 εἰ . . . ἀμείψω (minatory).
(4). 683 εἰ . . . νικήσει (cold indifference).
(5). 775 εἰ Ζεὺς θήσει (confidence).

EUMENIDES.

- (1). 491 εἰ κρατήσῃ, 'is to.' (1). 741 κὰν ἰσόψηφος κρείττῃ.
(2). 597 εἰ σε μάρψῃ (minatory).

SUMMARY.

	εἰ with fut. ind.	εἰάν with subj.
Prometheus Vincitus,	1	2
Septem contra Thebas,	2	2
Persæ,	2	1
Supplices,	6	0
Agamemnon,	4	2
Choëphori,	5	0
Eumenides,	2	1
	<hr/> 22	<hr/> 8
	73.33 per cent.	26.67 per cent.

SOPHOCLES.

AJAX.

- | | |
|---|---------------------------|
| (1). 313 εἰ μὴ φανοίην (= fut. ind.)
(minatory). | (1). 528 εἰαν... τοῖμα. |
| (2). 510 εἰ... δωίσεται, 'is to,' w. οἴκτειρε
(verb of emotion). | (2). 688 ἦν μόλῃ. |
| (3). 1155 εἰ ποιήσεις (minatory). | (3). 1068 κἂν μὴ θέλῃς. |
| (4). 1241 εἰ φανοίμεθα, 'are to.' | (4). 1184 κἂν μηδεὶς εἴῃ. |
| (5). 1248 εἰ ἐξωθήσομεν, 'are to.' | |
| (6). 1256 εἰ μὴ κατακτήσει (minatory). | |
| (7). 1308 εἰ βάλειτε, 'undertake to,'
(minatory). | |
| (8). 1314 εἰ πημανεῖς (as above). | |

ELECTRA.

- | | |
|--|---|
| (1). 245 εἰ... κείσεται, 'is to.' | (1). 554 ἦν ἐφ' ὧς μοι. |
| (2). 376 εἰ... λέξεις, 'have to say.' | (2). 593 εἰαν περ καὶ λέγῃς. |
| (3). 379 εἰ μὴ λήξεις (minatory). | (3). 821 χάρις μέν, ἦν κτάνῃ. |
| (4). 430 εἰ γὰρ μ' ἀπώσσει (minatory). | (4). 822 λήπη δ', εἰαν ζῶ. |
| (5). 465 εἰ σωφρονήσεις (stern warning). | (5). 938 εἰαν δέ μοι πίθῃ. |
| (6). 470 εἰ πείσεται (as above). | (6). 967 ἀλλ' ἦν ἐπίσπῃ. |
| (7). 503 εἰ μὴ... ἐν κατασχέσει (convic-
tion). | (7). 1053 οἷδ' ἦν σοδὸρ' ἱμείρουσα
τῇ χάνῃς. |
| (8). 582 εἰ γὰρ κτενοῦμεν, 'are to.' | |
| (9). 832 εἰ... ἐπιούσεις (remonstrance). | |
| (10). 1004 εἰ τις... ἀκούσεται (stern
warning). | |
| (11). 1044 εἰ ποιήσεις (as above). | |
| (12). 1210 εἰ στερήσομαι, 'am to be'
(emotion). | |
| (13). 1369 εἰ δ' ἐφ' ἔξειτον (stern warning). | |

OEDIPUS REX.

- | | |
|--|--------------------------------|
| (1). 54 εἰπερ ἄρξεις, 'are to.' | (1). 216 εἰαν θέλῃς. |
| (2). 233 εἰ σιωπήσεσθαι (minatory). | (2). 321 ἦν ἐμοὶ πίθῃ. |
| (3). 521 εἰ... κекήησομαι, 'am to' (= fut.). | (3). 341 κἂν ἐγὼ σιγῇ στέγω. |
| (4). 586 εἰ ἔξει, 'is to.' | (4). 461 κἂν λάβῃς. |
| (5). 620 εἰ... προσμενῶ, 'am to.' | (5). 605 εἰαν με... λάβῃς. |
| (6). 666 εἰ... προσάψει, 'are to.' | (6). 748 ἦν ἐν ἐξείπῃς ἔτι. |
| (7). 702 εἰ... ἐρεῖς, 'art going to.' | (7). 839 ἦν... εἰρενθῇ. |
| (8). 843 εἰ μὲν... λήξει. | (8). 1062 οἷδ' εἰαν... φανῶ. |
| (9). 846 εἰ δ'... αὐδῇσει (dread alterna-
tive). | (9). 1159 ἦν φράσω, διόλλυνμα. |
| (10). 889 εἰ μὴ... κερδανεῖ... ἱρξεται,
(stern prophetic tone). | |
| (11). 901 εἰ μὴ... ἀρμόσει (as above). | |
| (12). 1166 εἰ ἱρήσομαι (minatory). | |

OEDIPUS COLONEUS.

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|---|--|
| <p>(1). 628 εἴπερ μὴ ψείσονται (confident expectation).
 (2). 648 εἰ σοί γ'... ἐμμενεῖ 'is to.'
 (3). 827 εἰ μὴ πορεύσεται (minatory).
 (4). 837 εἰ τε πιμανεῖς (minatory).
 (5). 1181 καὶ παραινέσω (= μέλλω π.).
 (6). 1340 εἰ σὺ... ξυμπαραστήσει (confident expectation).
 (7). 1435 εἰ τελεῖτέ μοι (as above).
 [NOTE.—166 εἰ τιν' οἴσεις I omit as merely an old conjecture.]</p> | <p>(1). 457 εἰάν θέλητε.
 (2). 505 ἦν... σπάνιν ἰσχυς.
 (3). 814 ἦν σ' ἔλω ποτέ (minatory).
 (4). 862 ἦν μὴ... ἀπειργάσθην.
 (5). 1040 ἦν μὴ θάνω.
 (6). 1197 κὰν κείνα λείσσης.
 (7). 1210 εἰάνπερ σώζῃ
 (8). 1407 εἰάν αἱ τοῦδ' ἀραί τελῶνται.
 (9). 1443 δυστάλαινα... ἐγώ, εἰ σου στερη-
 θῶ (see above, page 8).
 (10). 1770 εἰάν πως διακωλύσωμεν.</p> |
|---|--|

ANTIGONE.

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|--|---|
| <p>(1). 59 εἰ... παρτίξομεν, 'are to.'
 (2). 90 εἰ καὶ ὀνήσεται.
 (3). 93 εἰ ταῦτα λέξεις (minatory).
 (4). 229 καὶ ταδ' εἴσεται (minatory).
 (5). 234 καὶ τὸ μηδὲν ἐξερῶ.
 (6). 307 εἰ μὴ... ἐκφανείτε (minatory).
 (7). 324 εἰ δὲ ταῦτα μὴ φανεῖτε (minatory).
 (8). 414 εἰ τις ἀφειδήσοι (= ind. in or. rect.) (minatory).
 (9). 462 εἰ... θανοῦμαι, 'must die.'
 (10). 485 εἰ... κείσεται, 'are to.'
 (11). 659 εἰ... θρήψω.</p> | <p>(1). 45 ἦν σὺ μὴ θέλῃς.
 (2). 87 εἰάν μὴ κηρύξῃς.
 (3). 327 εἰάν δέ τοι ληφθῇ τε καὶ μή.</p> |
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TRACHINIAE.

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|---|---|
| <p>(1). 305 εἰ τι δράσεις 'dost intend to.'
 (2). 666 εἰ φανήσομαι (after verb of emotion: semi-causal).
 (3). 712 εἰ τε μὴ ψενσθήσομαι.
 (4). 719 εἰ σφαλίησεται.
 (5). 732 εἰ μή τι λέξεις (Herm. οὐ μὴ τι λ.).
 (6). 1113 εἰ σφαλήσεται (expression of emotion).
 (7). 1246 εἰ τέρψεις 'art going to.'</p> | <p>(1). 411 ἦν εἰρεσθῆς.
 (2). 570 εἰάν πίῃῃ.
 (3). 572 εἰάν... ἐνέγκῃ.
 (4). 584 εἰάν πως ὑπερβαλῶμεθα.
 (5). 597 κὰν αἰσχρὰ πρᾶσσης.
 (6). 672 ἦν φράσω.
 (7). 1107 κὰν τὸ μηδὲν ὦ.</p> |
|---|---|

PHILOCTETES.

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|---|---|
| <p>(1). 66 εἰ δ' ἰργάσει (stern).
 (2). 68 εἰ... μὴ ληφθήσεται (as above).
 (3). 75 εἰ με... αἰσθήσεται.
 (4). 353 εἰ... αἰρήσοιμι (fut. opt. = fut. ind. εἰ almost = ὅτι: semi-causal).
 (5). 376 εἰ... ἀφαιρήσοιτο (as above).
 (6). 988 εἰ... ἀπάξεται, 'is to.'</p> | <p>(1). 52 ἦν τι... κλύης.
 (2). 126 εἰάν δοκῇτε
 (3). 479 εἰάν μόλω ζῶν.
 (4). 769 ἦν μόλωσι.
 (5). 817 ἦν προσθίγῃς.
 (6). 982 οὐδ' ἦν δράσῃ
 (7). 985 ἦν μὴ ἐρπῃς.</p> |
|---|---|

PHILOCTETES.

- (7). 1198 οὐδ' εἰ... εἰσι (an extravagant supposition which seems natural to a man in a frenzied state).
 (8). 1393 εἰ... δυνήσόμεθα, 'are to.'
- (8). 999 οὐδ' ἦν χρῆ.
 (9). 1259 κὰν... φρονύς.
 (10). 1298 ἐὰν... θέλῃ.
 (11). 1299 ἦν τοῦτ' ὀρθωθῇ.
 (12). 1342 ἦν... ψευστοῦ.
 (13). 1405 ἐὰν πορθῶσι.
 To which add opt. for subj. in or. obl.
 (14). 613 εἰ ἀγοιντο.
 (15). 618 εἰ μὴ θέλοι.

SUMMARY.

	εἰ with fut. ind.	ἐὰν with subj.
Ajax,	8	4
Electra,	13	7
Oedipus Rex,	12	9
Oedipus Coloneus,	8	10
Antigone,	11	3
Trachiniae,	7	7
Philoctetes,	8	15
	<hr/> 67	<hr/> 53
	54 92 per cent.	45.08 per cent.

EURIPIDES.

ALCESTIS.

- (1). 386 εἰ με δὴ λήψεις.
 (2). 700 εἰ τὴν γυναικα... πείσεις.
 (3). 704 εἰ δ' ἡμᾶς... ἐρεῖς (minatory).
 (4). 733 εἰ μὴ... τιμωρήσεται.
- (1). 56 κὰν γραῦς ὀλήται.
 (2). 846 κἄνπερ μάρψω.
 (3). 850 ἦν δ' οὖν ἀμάρτω.

ANDROMACHE.

- (1). 61 εἰ τις... αἰσθήσεται.
 (2). 254 εἰ μὴ... θανούμαι γε.
 (3). 314 καὶ μὴ... ἐρημώσεις (minatory).
 (4). 378 εἰ μὴ θήσομαι.
 (5). 409 εἰ σωθήσεται.
 (6). 708 εἰ μὴ φθερεῖ (minatory).
 (7). 925 εἰ δ' ἤξει πάρος.
 (8). 970 εἰ πέρσει πόλιν.
 (9). 1282 εἰ... οἴσεται (= μέλλει οἴσεσθαι).
- (1). 44 ἡ με κωλύση θανεῖν.
 (2). 90 ἦν τι καὶ πάθω.
 (3). 163 ἦν... θέλῃ.
 (4). 188 ἦν... κρατήσω.
 (5). 202 ἦν σὺν καὶ τέκῃ.
 (6). 338 ἦν... ὑπεκδράμω.
 (7). 381 ἦν... θάνῃς.
 (8). 414 ἦν ὑπεκδράμῃς μόρον.
 (9). 432 ἦν... θέλῃ.
 (10). 442 ἦν... θέλῃ.
 (11). 663 ἦν... μὴ τέκῃ.
 (12). 689 ἦν δ' ὄξυνθυμῆς.
 (13). 740 κὰν... κολάσῃ.
 (14). 999 ἦν... μείνωσιν.

BACCHAR.

- | | |
|--------------------------------------|--------------------------------|
| (1). 239 εἰ...λήψομαι (minatory). | (1). 50 ἦν...ζητῇ. |
| (2). 786 εἰπερ πεισόμεσθα, 'are to.' | (2). 311 ἦν δοκῆς. |
| (3). 1261 εἰ...μενεῖτε, 'are to.' | (3). 355 κἄνπερ λάβῃτε. |
| | (4). 640 κἄν...ἐλθῃ. |
| | (5). 817 κἄν ἐλθῃς λάθρα. |
| | (6). 823 ἦν ἀνὴρ ὀφθῆς. |
| | (7). 960 ἦν σὺ μὴ ληφθῇ πάρος. |

HECUBA.

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|--|--------------------------------|
| (1). 347 εἰ δὲ μὴ βουλήσομαι. | (1). 293 κἄν κακῶς λέγῃ. |
| (2). 802 εἰ...διαφθαρήσεται. | (2). 313 ἦν τις αὐτὸ φανῇ. |
| (3). 863 εἰ διαβληθήσομαι, 'am to.' | (3). 399 οἶκ, ἦν γε πείθῃ. |
| (4). 1233 εἰ τῷδ' ἀρκέσεις, κακὸς φανεί. | (4). 751 κἄν τυχῶ κἄν μὴ τύχῳ. |
| | (5). 870 ἦν τι βουλεύσω. |
| | (6). 872 ἦν...φανῇ τις. |
| | (7). 1006 ἦν σὺ κατθανῇς. |

HELENA.

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|---|---------------------------------------|
| (1). 919 εἰ διαφθέρεις, 'art going to.' | (1). 429 ἦν πως (semi-interrogative). |
| (2). 975 εἰ...συλήσετε (minatory). | (2). 479 ἦν...λάβῃ. |
| (3). 1011 εἰ μὴ ἀποδώσω. | (3). 506 ἦν...ὠμόφρων ᾗ. |
| (4). 1051 εἰ δὲ κερδανῶ. | (4). 508 ἦν...ἐνδοιδῶ. |
| (5). 1155 εἰ κρινεῖ. | (5). 743 ἦν δυνώμεθα. |
| (6). 1631 εἰ μὴ μ' ἐάσεις (minatory). | (6). 832 ἦν...μὴ ἀποδέξῃται. |
| | (7). 929 ἦν...δ' Ἑλλάδ' ἐλθῶ. |
| | (8). 980 ἦν...μὴ πόδ' ἀντιτῇ. |
| | (9). 1049 ἦν...λέξῃ. |
| | (10). 1071 ἐάνπερ...λάβῳ. |
| | (11). 1085 ἦν...ὄρῳ. |
| | (12). 1091 ἦν ἀλῶ. |
| | (13). 1176 ἦν γε δὴ ληφθῇ μόνον. |
| | (14). 1291 ἦν...ἐλθῶ. |
| | (15). 1292 ἦν γυνὴ γένῃ. |
| | (16). 1388 ἦν δυνώμεθα. |
| | (17). 1392 ἦν...δόξῳ. |
| | (18). 1394 ἦν...μὴ παρῆς. |
| | (19). 1425 ἦν διδῶς. |
| | (20). 1444 κἄν...θίγῃς. |
| | (21). 1637 ἦν λέγῳ. |

ELECTRA.

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|--|---|
| (1). 48 στένω...εἰ εἰσώψεται (emotional). | (1). 110 ἦν τι δεξιόμεσθα (semi-interrogative). |
| (2). 261-3 ταρβῶν—εἰ δὴ ποτ' ἤξει. | (2). 274 ἦν υἱόλῃ. |
| (3). 336 αἰσχροῦν, εἰ...μὲν ἐξεῖλεν...ὁ δ' οὐ δυνήσεται. | (3). 582 ἦν δ' ἀσπάσωμαι. |
| (4). 584 εἰ...ἔσται, 'is to be.' | (4). 638 ἦν θεὸς θέλῃ. |
| (5). 686 εἰ...πεσεῖ (solemn warning). | (5). 690 ἦν μὲν ἐλθῃ. |
| (6). 1093 εἰ δ' ἀμείψεται, 'is to.' | |

HERACLIDAE.

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|--|---------------------------|
| (1). 165-8 εἰ...ἐμυθήσει (stern warning). | (1). 47 ἦν...ἀπωθόμεσθα. |
| (2). 197 εἰ...ἔσται, 'is to be.' | (2). 67 κὰν σὺ μὴ θέλῃς. |
| (3). 243 εἰ...παρήσω. | (3). 158 ἦν πεπανθήσ. |
| (4). 272 εἰ μὴ...μαθήσεται (minatory). | (4). 253 ἦν δίκαιον ἢ τ. |
| (5). 373 εἰ...ἤξεις (minatory). | (5). 256 εἰ...μενῶ. |
| (6). 418 εἰ δὲ δὴ δράσω. | (6). 263 ἦν τι σωφρον ᾗς. |
| (7). 444 μέλει εἰ...τέρψω. | (7). 310 ἦν...φανῇ. |
| (8). 495 εἰ μὴ τι...ἐξαμηχανήσομεν (minatory). | (8). 342 κὰν θυραῖος ὦ. |
| (9). 593 εἰ γὰρ ἐξομεν. | (9). 424 ἦν δίκαια δρῶ. |
| (10). 652 εἰ...προσθίξει (minatory). | (10). 586 κὰν εὐρεθῇ. |
| (11). 739 εἰ δὴ ποῦθ' ἤξομέν γε τοῦτο γὰρ φόβος. | (11). 712 ἦν θάνῃς σί. |
| (12). 763 εἰ...παραδώσομεν, 'are to.' | (12). 714 ἦν...χρήσονται. |
| (13). 974 εἰ δράσεις τότε (minatory). | (13). 1020 ἦν θάνῃ. |

HERCULES FURENS.

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|-------------------------------------|--|
| (1). 210 εἰ...θανούμεσθα, 'are to.' | (1). 279 ἦν...δοκῶ, 'in case' (semi-int.). |
| (2). 490 εἰ τις...εἰσακούσεται. | (2). 848 ἦν πίθησθε. |
| (3). 1074 εἰ με κανεῖ (minatory) | (3). 1282 ἦν δὲ καὶ μένω. |
| (4). 1412 εἰ σ' ὄψεται (minatory). | |

SUPPLICES.

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|--|-------------------------------------|
| (1). 521 εἰ 'πιταξόμεσθα δὴ. | (1). 388 ἂν θέλωσι. |
| (2). 539 εἰ...τις ἐξεῖ, 'is to.' | (2). 389 ἦν δ' ἀπιστῶσι. |
| (3). 543 ταρβείτ' εἰ κινεήσονται χθονί. | (3). 397 ἦν σ' ἀπαλλάξῃ, 'in case.' |
| (4). 603 εἰ...φανήσονται. | (4). 473 ἂν...πίθῃ μοι. |
| (5). 711 εἰ μὴ στήσετε (solemn warning). | (5). 499 ἦν τε μὴ θέλῃ. |
| (6). 784 εἰ περ ὄψομαι, 'am to.' | (6). 541 ἦν τεθῇ νόμος. |
| | (7). 1194 ἦν...ἐλθῶσιν. |
| | (8). 1208 ἦν...ἐλθῶσιν. |

HIPPOLYTUS.

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|---|------------------------------|
| (1). 305 εἰ θανεῖ (stern). | (1). 505 ἦν λέγῃς καλῶς. |
| (2). 327 εἰ πείσει (minatory). | (2). 512 ἦν σὺ μὴ γένη κακ'. |
| (3). 461 εἰ μὴ στήρξεις. | (3). 995 οἶδ' ἦν σὺ μὴ οἴξ. |
| (4). 481 εἰ μὴ ἐνρήσομεν, 'are not to.' | |
| (5). 501 εἰπερ ἐκώσσει γε, 'is going to.' | |
| (6). 697 εἰ δέξει, 'wilt consent to.' | |
| (7). 938 εἰ...ἐξογκώσεται, 'is going to.' | |
| (8). 976 εἰ...ἡσηθήσομαι, 'am to.' | |
| (9). 1088 εἰ μὴ πείσει (minatory). | |

IPHIGENIA AULIDENSIS.

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|---|--------------------------------------|
| (1). 163 εἰ μὴ... εἰσιν. | (1). 32 κὰν μὴ σὺν θέλῃς. |
| (2). 296 εἰ προσαρμόσει. | (2). 150 ἦν... ἀντήσῃς. |
| (3). 654 εἰ σέ γ' εὐφρανῶ. | (3). 515 ἦν... ἀποστείλῃς. |
| (4). 817 εἰ τι δράσεις. | (4). 519 ἦν σάνῃ. |
| (5). 941 εἰ δι' ἐμ' ὀλεῖται, 'is to.' | (5). 533 κὰν... ἐκφύγῃ. |
| (6). 972 εἰ τις... ἐξαιρήσεται, 'undertakes to' (minatory). | (6). 915 ἦν τολμήσῃς. |
| (7). 1171 εἰ στρατεύσει (Elmsl.) | (7). 1007 ἦν σώσω. |
| (8). 1190 εἰ... εὐφρονήσομεν. | (8). 1016 ἦν δ' ἀντιβαίῃς. |
| (9). 1262 εἰ μὴ σε θύσω. | (9). 1025 ἦν... μὴ πράσσωμεν. |
| (10). 1268 θέσφατ' εἰ λύσω θεᾶς. | (10). 1134 ἦν γ' ἐρωτᾷς. |
| (11). 1415 ἀχθομαι... εἰ μὴ σε σώσω. | (11). 1166 κὰν τις σ' ἐρηται. |
| | (12, 13). 1271 κὰν θέλω κὰν μὴ θέλω. |
| | (14). 1344 ἦν δυνώμεθα. |
| | (15). 1421 ἦν ἐννώμεθα. |
| | (16). 1541 ἦν τaráξῃ. |

IPHIGENIA TAURICA.

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|---|---------------------------------|
| (1). 690 εἰ σε... κτενῶ. | (1). 100 ἦν... ληφθῶμεν. |
| (2). 975 εἰ μὴ με σώσει Φοῖβος. | (2). 337 ἂν ἀναλίσκῃς. |
| (3). 986 εἰ μὴ ληφθόμεθα. | (3). 754 ἦν καλῶς ἔχῃ. |
| (4). 999 εἰ... γενήσεται. | (4). 755 ἦν τι ναῦς πάθῃ. |
| (5). 1022 εἰ σέ σώσει, 'is to.' | (5). 762 ἦν ἐκώσῃς. |
| (6). 1034 εἰ κερδανεῖς, 'expectest to.' | (6). 764 ἦν... ἀφανισθῇ. |
| (7). 1412 εἰ μὴ... γενήσεται. | (7). 980 ἦν... κατὰσχωμεν. |
| | (8). 1010 ἦνπερ μὴ... πέσω. |
| | (9). 1219 ἦν... δοκῶ χρονίζειν. |
| | (10). 1230 ἦν νίψω φόνον. |

ION.

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|--|----------------------------|
| (1). 168 εἰ μὴ πείσει (minatory). | (1). 425 εἰς ἐὰν θέλῃ. |
| (2). 254 εἰ... ὀλοῦμεθα. | (2). 527 ἦν κτάνῃς. |
| (3). 375 εἰ... ἐκπονήσομεν. | (3). 595 ἦν... ζῆτῶ. |
| (4). 445 εἰ δ', οὐ γὰρ ἔσται, τῷ λογῷ δὲ χρήσομαι... δώσετε. | (4). 1037 κὰνπερ διέλθῃ. |
| (5). 648 εἰπερ... εὐτυχήσουσιν. | (5). 1130 ἦν... μένω. |
| (6). 669 εἰ μὴ... εὐρήσω. | (6). 1259 ἦν θάνῃς. |
| (7). 750 εἰ μὴνύσετε. | (7). 1309 ἦν... θέλῃς. |
| (8). 847 εἰ ὑφῆσεις... ἀπαλλάξει βίον. | (8). 1415 κὰν μὴ φράσω γε. |
| (9). 1024 καὶ μὴ κτενεῖς. | |
| (10). 1062 εἰ καὶρὸς ἀπεισι. | |
| (11). 1075 αἰσχύνομαι εἰ... ὕψεται. | |

CYCLOPS.

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|---|--------------------------|
| (1). 199 εἰ φευξόμεσθα, 'are to.' | (1). 217 ἦν θέλῃς. |
| (2). 307 εἰ... ἀναλώσεις (solemn remonstrance). | (2). 314 ἦν... δάκῃς. |
| (3). 474 εἰ... ἐκθύψομεν. | (3). 427 εἰς ἐὰν βοῦλῃ. |
| | (4). 573 κὰν μὲν σπάσῃς. |
| | (5). 575 ἦν δ' ἐκλίπῃς. |

MEDŒA.

- (1). 78 εἰ...προσίοισμεν, 'are to.'
 (2). 346 εἰ φευξοῦμεθα.
 (3). 352 εἰ...ὀψεται (minatory).
 (4). 381 εἰ ληφθήσομαι.
 (5). 512 εἰ φεύξομαι, 'am to.'
 (6). 577 καὶ...ἐρῶ, 'must.'
 (7). 931 οἶκτος εἰ γενήσεται τάδε.
 (8). 1249 εἰ κτενεῖς σφ' ὅμως.

NOTE.—184 φόβος εἰ! πείσω is interrogative. 1109, 1298 are doubtful.

- (1). 260 ἦν...ἔξενυρεθῇ.
 (2). 389 ἦν...φανῇ.
 (3). 391 ἦν...ἔξελαίνῃ.
 (4). 727 ἔάνπερ...ἔλθῃς.
 (5). 787 κἂνπερ...ἀμφιθῇ.
 (6). 1362 ἦν σὺ μὴ' γγελίς.

ORESTES.

- (1). 157 ὀλέεις, εἰ βλέφαρα κινήσεις (warning).
 (2). 239 εἰ μὲν εὖ (λέξεις).
 (3). 240 εἰ δ' εἰς βλάβην τιν'.
 (4). 272 εἰ μὴ' ξαμείψει (minatory).
 (5). 304 εἰ γὰρ προλείψεις.
 (6). 509 εἰ...ἀνταποκτενεῖ (?).
 (7). 511 ——— λύσει (?).
 (8). 566 εἰ...ῆξουσιν.
 (9). 599 εἰ μὴ...ῥύσεται.
 (10). 803 εἰ σε μὴ...ἐπαρκέσω, 'am to.'
 (11). 935 εἰ...ἔσται, 'is to.'
 (12). 940 εἰ δὲ δὴ κατακτενεῖτέ με.
 (13). 1102 εἰ τιμωρήσομαι.
 (14). 1106 εἰ γ' ἔσται καλῶς.
 (15). 1212 εἴπερ εὐτυχήσομεν.
 (16). 1533 εἰ...ἐπάξει.

- (1). 69 ἦν...μὴ...σωθῶμεν.
 (2). 308 ἦν σὺ κατανανῆς.
 (3). 559 ἦν λέγω.
 (4). 644 ἦν...σώσης.
 (5). 778 ἦν μένης.
 (6). 1148 ἦν μὴ σπασώμεθα (Dind. εἰ μὴ σπάσω μέλαν).
 (7). 1149 ἦν δ' οἶν μὴ κατάσχωμεν.
 (8). 1191 ἦν τι δρῆ.
 (9). 1195 κἂν μὲν σε σώζῃ.
 (10). 1198 ἦν δὲ...κτείνῃ με.
 (11). 1200 ἦν πολλὸς παρῇ.
 (12). 1218 φύλασσε ἦν τις φθῇ.
 (13). 1316 ἦν ἀλφ.
 (14). 1332 ἦν σὺ μὴ λέγῃς.
 (15). 1593 ἦν γε μὴ φύγῃς πτεροῖς.

RHEKUS.

- (1). 113 εἰ μὴ κνρήσεις (warning).
 (2). 169 εἰ τόδ' αἰτήσῃ γέρας.
 (3). 600 εἰ διοίσει (warning).

- (1). 118 ἦν ἄρα (verb implied).
 (2). 126 κἂν μὲν αἰρωνται.
 (3). 141 κἂν μὲν πυνθώμεθα.
 (4). 143 ἔαν δ' ἀπαίρωσι.
 (5). 521 ἦν τι καὶ δέῃ.
 (6). 572 ἦν δ' οἶν ἐγειρήσῃ.
 (7). 880 ἦν...πύθῃ.

TROADES.

- (1). 662 εἰ...ἀναπτίξω.
 (2). 735 εἰ γάρ τι λέξεις (solemn warning).
 (3). 890 αἰνῶ σε εἰ κτενεῖς (verb of emotion: semi-causal)

NOTE.—1249 εἰ...τεύξεσθαι may be considered interrogative.

- (1). 355 κἂν μὴ...ῃ.
 (2). 701 κἂν δρῆς τάδε.
 (3). 718 ἔαν λέγῃς.
 (4). 914 ἦν θάνω.
 (5). 904 κἂν εὖ...
 (6). 914 κἂν κακῶς δόξω.
 (7). 1059 κἂν ἐτ' ὥσ' αἰσχίονες.

Add

- (8). 874 }
 (9). 928 } opt. for subj. in *orat. obl.*
 (10). 930 }

PHOENISSÆ.

- | | |
|---|---------------------------------|
| (1). 19 εἰ γὰρ τεκνώσεις (minatory). | (1). 559 ἦν δ' ἐρωμαι. |
| (2). 244 εἰ τι πείσεται. | (2). 561 ἦν... νικήσῃ. |
| (3). 725 εἴπερ... σωθήσεται. | (3). 571 ἦν ἔλῃς. |
| (4). 885 εἰ μὴ λόγοις τις... πείσεται (minatory). | (4). 578 ἦν δ' αὖ κρατηθῆς. |
| (5). 1621 εἰ με... βαλεῖς. | (5). 629 κὰν τι γένηται. |
| | (6). 757 ἐάν τι... σφαλῶ. |
| | (7). 765 ἦν τύχῃ. |
| | (8). 775 ἦν περ κρατήσῃ. |
| | (9). 777 κὰν φίλων τις ᾖ. |
| | (10). 938 ἦν λάβῃ. |
| | (11). 975 κὰν μὲν φθάσωμεν. |
| | (12). 976 ἦν δ' ὑστερήσῃς. |
| | (13). 1216 ἦν μὴ γε... ἐκφύγῃς. |
| | (14). 1231 κὰν μὲν κτάνω. |
| | (15). 1280 ἦν μὲν φθάσω. |
| | (16). 1657 κὰν ἀπεννέπῃ. |

SUMMARY.

	ei with fut. ind.	ἐάν with subj.
Alcestis,	4	3
Andromache,	9	14
Bacchæ,	3	7
Hecuba,	4	7
Helena,	6	21
Electra,	6	5
Heraclidae,	13	13
Hercules Furens,	4	3
Supplices,	6	8
Hippolytus,	9	3
Iphigenia Aulidensis,	11	16
Iphigenia Taurica,	7	10
Ion,	11	8
Cyclops,	3	5
Medea,	3	6
Orestes,	16	15
Rhesus,	3	7
Troades,	3	10
Phoenissæ,	5	16
	<hr/> 131	<hr/> 177

42.53 per cent. 57.47 per cent.

NOTE.—At my request, two Fellows of the Johns Hopkins University, Messrs. A. D. Savage and J. H. Wheeler, undertook to revise the count, Mr. Savage reading half of Euripides, and Mr. Wheeler the other half and the whole of Sophocles. While the result has not been materially affected by the new count, I am glad to have the statistics brought nearer to absolute accuracy, and am happy to express my thanks to these scholars for their kind and intelligent assistance.

II.—On Grote's Theory of the Structure of the Iliad.

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GROTE'S theory of the structure of the Iliad is stated and defended in the second volume of his History of Greece, which was published in 1846. It was noticed in reviews of the new history in some English and American magazines. Most of these articles, I believe, I have seen, but I do not find in any of them a minute, careful examination of the new theory on a critical basis. The discussion is from a literary point of view and of a general character, containing little more than the expression of opinions. In Germany it seems to have been thought worthy of more thorough consideration. Several scholars whose names are familiar to students of Homer discussed it with different results. Friedländer published in 1853 a book in which he adopted and defended it. Düntzer in the *Jahrbücher für Philologie* (2d Supplementband, 1856) claimed to have already published nearly the same view, though he refused assent to the precise form which Grote proposed. Ribbeck attacked it from the Lachmann standpoint in *Philologus* (1853), aiming to show that Grote was wrong, not in throwing out what he did, but in regarding the rest as a self-consistent poem. Finally, W. Bäumlein in *Philologus* (1856) subjected the theory to critical examination from the conservative point of view, and concluded by rejecting it as merely one of many attempts, equally unsuccessful in his judgment, to break down the unity of the Iliad. Thus being approved by some and rejected on conflicting grounds by others, it seems still fairly open to discussion. It may indeed be thought that the long silence of English scholars on the subject is an argument for its own continuance, as showing that no one cares to have it interrupted, but, as the theory confronts every student of Homer or reader of Grote's history, it may be reasonable to suppose that some would be interested or profited by a discussion of it. I ought perhaps in the outset

to say that I have no new theory of the Iliad to propose. My purpose is simply critical, or, if you please, destructive; it is to take up the statements and arguments of Grote and see if they bear examination. I do not mean to argue either for or against the unity of the poem. Into those larger questions of the authorship and integrity of the poem I do not enter. Of course I shall not knowingly rest any argument on a line or passage which is reasonably or generally suspected. Nor shall I try to evade an argument by suggesting new suspicion of the text. Here is the poem, which certainly received its present form from somebody, though we know not who it was or when he did his work. Our question may really be only as to the success or failure of a compiler or editor, but that is a fair subject for discussion in connection with a theory which aims to go behind such a man's work and show his materials uncombined; does the poem as it stands admit this analysis? In my examination of the subject I have made great use of Bäumlein's article above mentioned, and wish to acknowledge fully my debt to him. Part of the same material is also to be found in Bergk's *History of Greek Literature*, but in the general plan and many particulars I have followed Bäumlein.

Grote's theory may be stated best in his own words (*History of Greece*, II. p. 175 f. Am. edition):

"The first book, together with the eighth, and those from the eleventh to the twenty-second inclusive, seem to form the primary organization of the poem, then properly an Achilleis; the twenty-third and twenty-fourth books are perhaps additions at the tail of this primitive poem, which still leave it nothing more than an enlarged Achilleis. But the books from the second to the seventh inclusive, together with the tenth" (and here he might have added the ninth also), "are of a wider and more comprehensive character, and convert the poem from an Achilleis into an Iliad. The primitive frontispiece, inscribed with the anger of Achilles and its direct consequences, yet remains, after it has ceased to be co-extensive with the poem. The parts added, however, are not necessarily inferior in merit to the original poem: so far is this from being the case, that amongst them are com-

prehended some of the noblest efforts of the Grecian Epic. Nor are they more recent in date than the original: strictly speaking, they must be a little more recent, but they belong to the same generation and state of society as the primitive Achilleis."

In support of this theory, the first statement made is that in the books of the original Achilleid "the sequence of events is more rapid, more unbroken, and more intimately knit together in the way of cause and effect" than in the others. If this difference is tested by the impression made upon a reader by the two parts of the poem, there would be room for difference of opinion. It is hardly possible to expect general agreement in such a matter. If it be tested by the number of incidents crowded into a day, the difference is not very great. Following Faesi's analysis, there are twenty-one days covered by the first book, the twenty-second day covers five books and nearly a sixth (ii. 1-vii. 380), the twenty-third and twenty-fourth days some fifty lines each, then the twenty-fifth day three books (viii.-x.), the twenty-sixth day eight books (xi.-xviii.), the twenty-seventh day four books and a few lines more (xix. 1-xxiii. 108), then two comparatively short or empty days, and some twenty in the last book. Of course these long periods in the first and last books are made up by groups of ten or twelve days which are mere intervals in which nothing is narrated. Throwing these out, the other single days cover from fifty to some five thousand five hundred lines each, and all differ in length. In this point of view not much distinction can be made between the parts of the poem which Grote regards so differently. But let us grant that there is a distinction of the kind he suggests; does it prove difference of origin or relation to the whole poem? Are not such differences in the rate of movement to be looked for in any poem on so large a scale? Look at the *Odyssey*. There the distribution of days is much more even—many of them covering about the length of an average book. Only the fifth book corresponds to the first and last of the *Iliad* in containing in its limits twenty-one days. But there are two pivots of the action, two important points, one the narrative

of Odysseus's return told by himself, the other his arrival in his own house and vengeance upon the suitors. At each of these the time is crowded; the first, occupying one night, covers five books (viii.-xii.), and at the second the events of two days extend over seven books (xvi.-xxiii.). This parallel case, where Grote admits no division, illustrates the scheme of days in the *Iliad*. The introduction and the close cover between them forty-two days in two books, where long intervals, of little action for the purpose of the poem, are condensed into a few lines. Of the intervening eight days, four are briefly dispatched, but the other four, containing the struggles of the Greeks without Achilles, his return, and the killing of Hector, belong to the crises of the story and cover respectively three, five, six, and eight books. There is in this respect nothing true of the *Iliad* which is not equally true of the *Odyssey*, and nothing true of either which would not be equally true of any poem expanding a single selected point in a long story. As to the alleged impossibility of stopping anywhere between books eleven and twenty-two, so as to break apart the separate songs, that may trouble one of the Lachmann school but not one who discusses the poem as a whole. Mr. Grote goes on to say that there could never have been a separate poem called *Patrokleia* ending with the death of Patroklos, because he is so subordinate a character, "standing to Achilles in a relation of dependence, resembling that of Telemachus to Odysseus." The illustration is perhaps unfortunate, because recent investigators have regarded a *Telemacheia* as one of the constituents of the *Odyssey*. The remark itself, however, is open to question. What Patroklos did in his brief flash of action might as well be a distinct song, one would think, as the achievements of any hero in the long war. If we had not the *Iliad* and *Odyssey*, some critic might well suggest that no such poem as these could be constructed on the events of ten or twenty days in a war of ten years and a subsequent wandering of another ten.

The next general argument that Grote adduces is summed up in a few words on the next page (178) of the history, that "the consequences of the anger of Achilles do not appear

until the eighth book." In other words, the poet, after concentrating our attention in the first book on that anger and the consequent promise of Zeus to Thetis, seems to lose entirely from his thought for the next six books the fulfilment of that promise. This remark seems to rest upon the idea that a purpose thus formed and expressed must be put into immediate, if only partial, execution. Such an idea holds good, one may say, as what ought to be in the sphere of morals; but does it apply to poetry? Is the method of art conformed to it? Is it not rather the artistic plan, adopted from the experience of life, to defer the fulfilment of a promise or a purpose, to interpose and remove obstacles one by one, to vary the success on both sides in a conflict, to make the finally defeated party prove itself hard to defeat? All these things contribute to the suspense and so to the interest in the hearer's or reader's mind, and seem essential to the building up of a story in due proportion. Nor does this degree of artifice belong to a more developed stage of art than that in which the *Achilleid* must have been composed. The poet who imagined that dispute between Achilles and Agamemnon in the first book, with its skilful delineation of the growing anger in the words of each from vague to precise threats, from sarcasm and taunt to defiance and denunciation, was certainly equal to such simple and natural development of the subsequent plot. A similar delay, too, occurs at the end of the *Achilleid* when Achilles declares (xviii. 114 f.) a purpose to go at once to find and slay Hector, but one thing after another comes in to put off the execution of that purpose, so that it does not come until late the next day, in the twenty-second book. This I mention only to show of what the poet's art was capable.

Again it may be remarked that such prompt fulfilment as Grote requires of the promise of Zeus is not in harmony with the character of the God and of his government as shown in any part of the Homeric poems. He does not in either poem work out promptly, firmly, and openly a plan which he has independently and wisely formed. He is not, in the Homeric conception, a being strong enough to have in himself the

ultimate decision of questions on which the gods are divided. There is a fate dimly shadowed behind him, and when by the materialized device of a balance he learns what that fate is in a given case, he must execute it, though his heart is filled with pity for the losing side. Even then he acts irregularly and inconsistently, somewhat like the conventional oriental despot who gets things done somehow when he insists upon it, but has not the intellect or the energy to inspire obedience and secure efficient government. He alone knows certainly what the fate is, and with this knowledge and a consciousness of power, he lets the other gods fight among themselves, laughs at their wounds, and indolently allows them to outwit him and almost succeed in thwarting his plan, but always in the end has his way and executes the fate. Even in the Achilleid, after his promise to Thetis, a prayer from Agamemnon touches his pity and he sends (as again in xiii. 821 f.) a favorable prodigy which gives the Greeks a temporary success (viii. 246-252). Again, in book xiv. he lets himself be seduced by Hera into a sleep, during which Poseidon stimulates the Greeks so that they drive back the Trojans from the ships and Hector is carried off stunned and senseless. In the thirteenth book, too, still within the Achilleid, Poseidon without the knowledge of Zeus secures a success for the Greeks on the left wing, while Zeus himself inspires the Trojans in the centre of the line; but it is Hector without help from Zeus who finds out and in a measure repairs the injury caused by Poseidon. His favor varies from one side to the other even in book xvii., in the battle over the body of Patroklos. Grote himself notices the inconsistencies in the action of Zeus, but only at one point where they afford him an argument for his theory. He speaks (p. 189) of the attitude of Zeus at the beginning of the fourth book as irreconcilable with that in the first and eighth books, saying that he "discusses nothing but the question of continuance or termination of the war, and manifests anxiety only for the salvation of Troy," forgetting all about his promise to Thetis. He seems in this criticism to overlook entirely the words with which the first speech of Zeus in book iv. is introduced:

*αὐτίκ' ἐπειρᾶτο Κρονίδης ἱρεθίζεμεν Ἥρην
κερτομίῳς ἐπέεσσι, παραβλήδην ἀγορεύων*

"Zeus at once attempted to exasperate Hera with taunting words and deceitful speech." This plainly means that the speech which follows is designedly insincere and may be expected to conflict with his words and deeds elsewhere. And when in the next speech he reproaches Hera with her hatred of Troy, but still, though unwillingly, consents to her desire to destroy it, there is nothing in that inconsistent with his promise to Thetis, for the fulfilment of that promise by bringing Achilles again into the field secured the victory of the Greeks. In thus requiring that Zeus should begin at once to make his taking the Trojan side perceptible, Grote seems then to require something not in keeping with the character of the god as conceived by the poet, nor yet with his action in the Achilleid itself.

Again, it is not easy to see what valid objection there is to the common conception of the Iliad, that it was in the intent of the poet, as far back as we can see, to bring out in the first half of the poem the inability of the other Greek heroes to make good the absence of Achilles. It is certainly worth observing how carefully this purpose seems to be carried out, in bringing forward each one of the principal heroes in turn to bear the burden of the fight. It may be said indeed that these separate instances are interpolations, or rather enlargements of the poem by subsequent additions of whole books, in the interest of other chiefs. That may perhaps be true of the tenth book, for instance, which most critics agree in regarding as a later addition. But on examination it does not appear that these ἀπιστεῖαι of other chiefs are confined to the part of the poem which Grote would reject. Menelaus is made prominent in iii. and xvii., Diomedes in v., Aias Telamonius in vii. and xv., Agamemnon has his turn in xi., and Patroklos in xvi. All these displays of valor are actually conditioned upon the absence of Achilles from the field, and are at the same time poetically conditions of his return to it. They are actually conditioned upon his absence, for apparently there would be no occasion or room for them in the story, were he

present. They are poetically conditions of his return, because as the poem stands, it seems that the poet designedly postponed his return until after they had all been worked into his plot. Now how does Grote deal with this view of the relation of these books to the rest? That they contain the "attempts of the other heroes to compensate for the absence of Achilles," says he, "is noway borne out by the poet himself. From the second to the seventh book, Achilles is scarcely alluded to. * * * He is mentioned two or three times as absent, and Agamemnon (ii. 377) regrets the quarrel, but we never hear any such exhortation as 'Let us do our best to make up for the absence of Achilles.'" (Note, p. 192.) As to this last remark, it is true there is no such exhortation to be found in those books, but it is also true that there is none in the much longer passage of the poem included in book viii. and books xi.-xvii. inclusive, that is, in the whole Achilleid before his return—so that the remark should have no force as an argument against books ii.-vii. The only approach to such a remark is made when Hera in v. and Poseidon in xiii. and xiv. exhort the Greeks not to let the absence of Achilles make so much difference in their success. As to the frequency of reference to Achilles, there are twenty places where his name occurs in the three thousand four hundred lines of these books, and in the books of the Achilleid in which he does not take part in the action (viii., xii.-xv.), containing three thousand one hundred lines, there are only fourteen references to him. Grote further remarks that these books (ii.-vii.) show "not the insufficiency of all the other heroes without Achilles, but the perfect sufficiency of the Greeks under Diomedes, Agamemnon, etc., to make head against Troy." The object of the Greeks however was not to make head against Trojan attack, but to capture the city by defeating utterly the Trojan army, and this they cannot do in these books—nor do they make any appreciable progress towards it. There is very little fighting, comparatively, in these books, for there is none at all in ii., and almost none in iv., while iii. and vii. are taken up with the two duels of chosen champions, and vi. with Hector's visit to Troy. So the only book full of fighting is

the fifth, where the Greeks certainly have the advantage on the whole, but not more than they do afterwards at the beginning of xi., until their chief heroes are wounded. Of course, in speaking of the relative valor and success of the two parties in the Homeric battles, one must not leave out of view the influence of the gods, which is continually exerted, first on one side, then on the other. It may truly be said, I think, that no hero gains striking advantage on either side, without an explanation of it by reference to the help of a deity; and hence to trace the varying successes of the two sides, it is only necessary to note the intervention of the divine power here or there. Grote seems to lose this fact out of sight for the moment when he speaks (in the same note) of the glory of Diomedes as beyond that of Achilles; for the successes of Diomedes are due to the presence of Athene in his chariot, stop when she leaves him, and never approach to the feat of Achilles in killing Hector. The want of prompt fulfilment of the promise of Zeus to Thetis is due simply to the intervention of other deities which Zeus allows or which for a time goes on without his knowledge, and it is not confined to books ii.-vii. The change from Greek success to Trojan success is gradual, but it must begin to be perceptible somewhere. Why should not that first marked success come in the eighth book, without casting suspicion on the six books that precede it? Athene, as has been mentioned, gives the Greeks their success in v. Then in viii. she and Hera start from Olympus to help them again, but are intercepted and sent back by Iris as the messenger of Zeus. After that they do nothing directly to aid the Greeks, who suffer in consequence. But Hera in xiv. seduces Zeus to slumber, and meanwhile Poseidon does his part to put off the fulfilment of the promise to Thetis by helping the Greeks most actively. Previously too in xi. Agamemnon has driven the Trojans back from the camp to the very walls of Troy, and Zeus himself sends word to Hector not to risk a conflict with him, but to keep out of the fight until Agamemnon is wounded and withdraws. Thus Zeus himself, as has been pointed out before, allows a temporary triumph to the Greeks within the limits of the Achilleid,

a fact which shows that similar successes of the Greeks in books ii.-vii. ought not to be used to cast doubt on the genuineness of those books.

The next point to be considered is the criticism of the way in which this enlargement, as Grote considers it, is connected at each end with the rest of the poem. At each place there is in his opinion an awkwardness or difficulty which marks the union of old garment and new cloth. At the beginning of the second book the awkwardness is that the dream, and its false message "produce no effect. For in the first place Agamemnon takes a step very different from that which his dream recommended, and in the next place, when the Grecian army is at length armed and goes forth to battle, it does not experience defeat, but carries on a successful day's fight." To test the justness of this criticism (which, by the way, betrays the same desire to have results brought about at once which has already been noticed), let us ask: Why does Zeus send the dream at all? Evidently to induce Agamemnon to do something which without the dream he would not be so ready to do—that is, of course, to lead out his army to battle. Why should Agamemnon need such encouragement to do this, but for the plague and the retirement of Achilles? This incident then grows naturally out of the first book, and something like this was almost necessary to bring on the fighting in which sooner or later the Greeks were to feel the need of Achilles. It resembles very closely the treacherous suggestion made by Athene to Pandaros, whereby the contest is reopened in breach of the truce, in the fourth book. The dream then plainly produces its effect, though not immediately. Agamemnon trifles with the army, and, when they take in earnest his proposition of an instant return home, the intended effect of the dream is almost defeated; but as soon as that mistake is corrected the designed consequences do follow in the marching out of both hosts to battle. Again the duel between Menelaus and Paris postpones the general combat, but, after that is over, on the same day comes 'the fighting of books iv.-vii. There is no fighting at all mentioned in the first book, and only a single passage (490 ff.), where Achilles is

said to abstain for twelve days from going to the agora or into battle, may be thought to imply it. How then should the poem go on from that first book? As it does in the eighth book, Grote would answer. But then at the very beginning of that book we find Zeus (10-17) forbidding the gods to aid either party—which command Athene (33-37) at once interprets as unfavorable to the Greeks, but promises to obey. This implies previous participation of some gods in the fighting, which has occurred with the express consent of Zeus in iv. and v. but has not been at all suggested if books ii.-vii. are omitted. In a note (p. 185), Grote modifies his opinion so as to include the dream, taking in the first forty-seven lines of the second book into his Achilleid, and going on from there to the eighth book. This is including, however, a little too much, for the last six lines of these forty-seven describe Agamemnon's dressing himself, and represent him as putting on his ordinary clothes for peaceful life, throwing his sword over his shoulder, and taking his sceptre in his hand. This is all right for the second book, where he goes out to call the Greeks to an assembly. But in the eighth book the first thing told of him is that in the midst of battle, about noon, he with other chiefs is terrified by the hostile thunder of Zeus, and retreats before the enemy. Now that description of his going forth in peaceful array to the ships of the Greeks is certainly left purposeless, and is inconsistent with what follows, if we go on from there to the eighth book. Suppose these six lines be dropped, and we read on from ii. 41 into the eighth book, then the difficulty remains that the dream should be so narrated and no subsequent reference ever made to it. In the prayer of Agamemnon himself in viii. 228-244, which he introduces by reference to former vows and boasts of the Greeks and bases upon his past offerings at the altars of Zeus, there is no allusion to this dream. The poet may well have forgotten the dream since three thousand five hundred lines have intervened, but could he at the distance of only two hundred lines?

But what shall be said of the false suggestion made by Agamemnon to the assembled army, which becomes the point

of junction between Achilleid and Iliad, if the Achilleid went on from ii. 41 (or 47) to viii. 1? How can that be explained? Grote says the object was to introduce the splendid picture of the sudden breaking up of the assembly and the interference of Odysseus to bring the men back, together with the episode of Thersites. Thus he intends, if I understand him, to suggest that this part was an addition when the enlargement was made. Surely this is not a necessary and hardly a probable inference. Why should not such a scene be introduced and in such a way by the supposed poet of the Achilleid? In what respect is this picture out of place in a poem devoted chiefly to Achilles, any more than the scene at the close of the first book, where Hephaestos acts as cupbearer among the gods, or the elaborate description of Agamemnon's arming at the beginning of the eleventh book? That it comes in just here does not indicate a juncture here of old and new, for there seems to be but one other place in the Achilleid where it could fittingly come, and that is at i. 54. If one may offer a suggestion, it seems more probable, to work back from effect to design, that the poet's object in introducing the scene at all was to increase the glory of the Greek heroes, by showing with what difficulties they had to contend in their own followers—to bring more clearly before the mind the effect on the army of its nine years of fruitless war and absence from home. This, it is true, widens the view to take in the whole war, but not any more than do the *ἀπιστεῖται* of Agamemnon in xi. and Menelaus in xvii., and the battle of the gods in xx.

Again, at the close of the seventh book, Grote finds similar difficulty in the construction of the wall around the Greek camp, which seems to him to reveal the hand of an enlarger. "As the poem now stands," says he (p. 186), no plausible reason is assigned why "the wall should be built." "Nestor proposes it without any constraining necessity, for the Greeks are in a career of victory," etc. This is certainly too strong a statement. There seems to be sufficient reason for anxiety on the part of the Greeks in the utter disappointment of the hopes with which they entered the battle. These hopes are expressed by Agamemnon in ii. 37, 413-18, and by Nestor in

ii. 436, being founded on the recent dream and looking to the capture of Troy on that day. Instead of such a triumph, though they had fought well and Diomedes by the present aid of Athene had achieved wonders, yet they had gained nothing, and repeatedly (v. 701, vi. 107 ff., vii. 17) they are spoken of as hard pressed and giving way. They are not in a "career of victory," but just holding their own against attack and in danger of being driven back to the ships (iv. 247 f., v. 791). Grote adds: "The Trojans are making offers of compromise which imply conscious weakness." Now this offer of compromise is expressly founded (vii. 351) by Antenor, who proposes it in the Trojan assembly, on the recent violation of the sworn truce by one of his own party, which in his view injures their prospects of success. Priam, apparently having no idea that the Greeks will accept the offer, adds to it a proposition for a truce until the dead are disposed of, with the expressed (vii. 377) intention of renewing the fight after it. But *before this message comes*, Nestor has made in the Greek assembly (vii. 337-43) the proposition to build a wall for the protection of the ships and themselves. And there is no hint in the answer of the Greeks to the Trojan message that they detect a confession of weakness in it. Still, granting that the proposal of a compromise and truce does imply conscious weakness in the Trojans, the fact that Nestor's suggestion of the wall precedes the coming of that proposal is a sufficient answer to the asserted inconsistency between them.

Grote goes on to show how on his theory the mention of the wall-building came to be introduced here. He supposes it to have been introduced because after the "brilliant scenes" in the books from the second to the seventh, it would surprise any one to find a wall spoken of at once in the eighth without any mention of its construction or previous existence, while in the Achilleid, passing at once from the first to the eighth book, there was no such previous success of the Greeks to make the existence of a wall seem strange. In his words, "since the Achilleis immediately after the promise of Zeus to Thetis went on to describe the fulfilment of that promise and the ensuing disasters of the Greeks, there was nothing to

surprise any one in hearing that their camp was fortified." It does not seem quite clear how disasters, following upon and caused by a promise, should make the existence of a previous wall seem natural. The connection between disasters and promise would rather suggest that there had been no disasters and hence no need of a wall before the promise was made. In the first book there is no mention of the wall, though it might well have occurred, especially at 344, where Achilles says Agamemnon cannot plan for the future how the Greeks shall fight safely by their ships, and again at 409, where he asks Zeus to help the Trojans to drive the Greeks to their ships and the water's edge. In the Achilleid the first mention of the walls would be at viii. 177, where Hector speaks with contempt of them. His reference to them agrees better with their recent erection. "I am sure," says he, "that Zeus has appointed victory for me, but woe for the Greeks; fools, who, you see, were building these walls here, weak things, not worth a thought." He would hardly speak so if the walls had stood nine years, although, as a friend suggests to me, this contemptuous language just here may be ascribed to the excitement of success which would make the walls seem now a trifling obstacle in his way. Another reference to the walls as just built occurs in ix. 348-51, but Grote rejects that book. In regard to this wall-building, I do not undertake to defend the seventh book as it stands; I have only tried to show that Grote's criticism of it is open to objection. But there are other difficulties about it, which give probability to Bergk's (*Hist. Gr. Literature*) recognition of a later hand in the form of its account of the building of the wall.

I have now discussed the principal points which Grote urges in support of his general theory, passing over his treatment of the ninth book, because one might accept what he says of that book without accepting his theory of an Achilleid. But I do not wish to stop without noticing that part of his discussion, because it seems open to criticism, and the topic itself is interesting to all students of the *Iliad*. Grote's reasons for rejecting the ninth book may be briefly stated. There are passages in the eleventh, thirteenth, and sixteenth books, in

the mouths of Achilles, Patroklos, Nestor, and Poseidon, which not only wholly ignore, but are inconsistent with, such a previous embassy carrying full apology and offers of restitution and compensation from Agamemnon to Achilles. Nothing more than such an offer is asked by Achilles from Thetis and by her from Zeus in the first book, so that this ninth book ought to end the poem if it belonged with the first. The terror of Agamemnon in the ninth book is neither accounted for by any previous disasters of the Greeks, nor consistent with his valor in the eleventh book. And finally the refusal of the offers by Achilles carries his pride and egotism to an excess such as to shock the Greek sentiment of Nemesis. These last two points may be taken up first, as I shall treat them briefly. The disasters of the Greeks in the previous books do not account for Agamemnon's "abject terror" in the ninth book. Elsewhere Grote has admitted that the disasters of the Greeks begin in the eighth book, and the fact appears strongly emphasized by the encampment of the Trojans outside the wall with the purpose, avowed by Hector, of preventing the Greeks from escaping in their ships under cover of the night. This is the very thing suggested by Agamemnon in the beginning of the ninth book, and for a long time no one knows what answer to make. Moreover it is not he who proposes the embassy, but Nestor, and Nestor too has already proposed, apparently for the first time, the stationing of a guard about the camp. That the fear of Agamemnon here is inconsistent with his gallantry in the eleventh book, shortlived as that display is and solitary, ought not to surprise any one who observes how all the heroes, except perhaps Aias and Diomedes, change often from one extreme to the other. That the obstinate refusal of Achilles to be reconciled to Agamemnon goes "beyond even the largest exigencies of insulted honor" is what one cannot so surely determine for another age and a different race of men. Such nursing of revenge might easily be paralleled from fact or fiction, and certainly cannot be ruled out from the possible conceptions of a Homer by the standard of a scholar and philosopher. And if it shocked the Greek sentiment of Nemesis, as well it might,

do we not find in the poem that sentiment justified by the death of Patroklos, which would not have occurred had Achilles yielded sooner? I know it is denied that this can be regarded as a penalty inflicted on Achilles; I know it is not expressly so explained anywhere in the poem; but I can hardly think any one can read the *Iliad* or hear its story told without so connecting the two events in his mind, even if the ninth book is left out, and much less if it is included. It may be here observed that the idea of Nemesis is much more clearly formulated and recognized in the *Odyssey* than in the *Iliad*, and in the later Greek poetry than in either; and this may account for the absence of direct reference to it in this connection.

It is argued further by Grote that nothing more is asked by Achilles and Thetis of Zeus in the first book than "that Agamemnon and the Greeks may be brought to know the wrong they have done their capital warrior and humbled in the dust in expiation of it." This he regards as accomplished in the ninth book, so that the further progress of the poem after it is inconsistent. Now what Achilles really asks in i. 408-12 is that by the aid of Zeus the Trojans may drive the Greeks to their ships and the water's edge, slaying them, so that the Greeks and Agamemnon may fully learn their folly in insulting him. In xvi. 61 ff. the same idea recurs. "I intended," says he, "not to lay aside my wrath until the din of battle should have come to my ships." It may be added that this phrase "to my ships" does not occur in the first book, but is to be found in the formal answer sent to Agamemnon, at the extreme of Achilles's wrath, in ix. 650 ff. Now the situation in book viii., though bad enough, as has been said, to justify great disappointment and anxiety on the part of Agamemnon, is not yet bad enough to satisfy that fixed terminus of wrath on the part of Achilles. The Trojans with inspiration from Zeus (viii. 335) drive the Greeks back to the trench and wall and are encouraged to encamp on the field, but they do not yet pass the wall or threaten the ships. Hence the remark of Grote (note, p. 182) that the subsequent defeats of the Greeks are causeless cannot be defended. There is no suggestion in the prayer of Achilles in book i. of compensation

or restitution of Briseis, and so the offers made in the ninth book do not satisfy him—he waits for something else. Now the whole tenor of the ninth book is in entire harmony with these expressions of Achilles in i. and xvi., and constitutes an explanation or expansion of them as well as of the *μηρὸς οὐλομένην* of the first line of the whole poem, without which expansion, one may boldly say, we should only half understand the conception of the poet. It may be not original, but if not, we must admit that some one else saw better than the first poet what was involved in those words and thus developed it. The speech of Achilles in rejecting the offers of the embassy is a most complete and powerful expression of such a state of mind as is only suggested by his words in i. and xvi. His love of glory in war is gone altogether for the time (316–322). He chooses (393–415) a long, peaceful, quiet life, instead of the brief but glorious career which he decides for in xviii. He dwells continually on the old wrong done to him, repeating it over and over in almost the same words (335 f., 344, 367 f., 375, cf. 646 f.). In the view of such a passion gifts are nothing, and he labors in his language to express fully his contempt for them: not ten or twenty times as many as are now offered, not all the wealth of Orchomenos or Egyptian Thebes, not gifts countless as the sand or the dust (379–85) will pacify him until—what? He repeats here, with this intense emphasis, the idea expressed more precisely in i. and xvi. “Until Agamemnon shall discharge for me the whole bitter outrage” (ix. 387). That line, in that connection, cannot be explained unless by Achilles’s own words, in the Achilleid itself, until the suffering of the Greeks from his absence had reached the limit his passion had fixed. This is the only adequate conception of the “accursed wrath” of Achilles. If we compare with this his speech in xvi., where he gives up in a measure his original purpose and consents to let Patroklos lead his men to battle, we find the same feeling there. He repeats there in 54 and 58 his complaint in nearly the same words. He repeats, as was mentioned above, the same intended limit to his inaction. But he does not speak so strongly in other respects, both because he is addressing his

friend, not the messengers of his enemy, and because he is now laying aside in a measure his former wrath. The rest of the speech will be considered farther on in another connection. This then is the answer to Grote's argument that Achilles asks nothing more than the humiliation of the Greeks and the return of Briseis, and that therefore, to be consistent with himself, he ought to accept the offer made in the ninth book. He does ask something more in i., as he himself says he had in xvi., and with these two passages of the Achilleid his conduct in ix. is consistent, and these are the only places in the poem where he suggests a limit to his wrath.

We take up now the subsequent passages which seem to Grote irreconcilable with the previous occurrence of the embassy of the ninth book, and which constitute the strongest argument against that book.

The first of these is in xi. 609 f., where Achilles, seeing a wounded man borne to Nestor's tent, calls Patroklos to go and learn who it was, and says to him: "Now I think the Greeks will come to my knees in supplication, for their need is no longer to be borne." It cannot be denied that it is strange that in this remark there is no reference to the ninth book; he would naturally say "will come *again*," if the ninth book had preceded. It does not seem, however, "a glaring inconsistency," as Grote calls it, for it appears from the ninth book that Achilles expected to be approached again and again in the same way. At the beginning of his address in answer to Odysseus he says (ix. 309 ff.): "It is best to declare my purpose to you bluntly, * * that ye may not come one after another to sit down and coax me." His remark in xi. then seems to mean that he expects what those words in ix. were designed to prevent.

Again in xi. 656-803, in Nestor's appeal to Patroklos to urge Achilles to relent at least so far as to let Patroklos take his armor and go into battle, Grote finds an argument against the ninth book in the absence of any reference to the offers there made. But what place is there for such reference? Remorse for the rejection of that offer would hardly move Achilles if the present disasters of the Greeks did not. And

Nestor had no authority to renew the offer without consulting Agamemnon. Furthermore, there is in the speech a passage which seems to allude distantly, as would be natural, to the fact that an attempt to persuade Achilles had been made and failed. After reminding Patroklos how his father Menoetios had directed him to influence Achilles with good advice, Nestor adds (xi. 790 ff.): "but even now remind him of this, if perhaps he may yield. Who knows but that with a god's help you may move his mind by counsel? There is power in the persuasion of a friend." These words gain meaning if they are taken as contrasting this appeal from a friend with the failure of the formal embassy.

Next comes, in the thirteenth book (95-124), the exhortation of Poseidon to the Greeks in battle, in which, admitting the injury done by Agamemnon to Achilles, he says: "even if he is to blame, it is not for us to be slack in fighting." Then follows a line (115) which Grote translates: "let us make an effort to heal the sore; the minds of good men admit this healing process": ἀλλ' ἀκέωμεθα θᾶσσον ἀκεσται τοι φρένες ἐσθλῶν. The rest of the speech refers only to courage in battle. Grote regards this line as quite inconsistent with the supposition that an attempt to "heal the sore" had already been made in the ninth book without success. It would be so certainly, but is the line to be understood as he translates it? His idea seems to rest upon a suggestion made by Heyne in his notes on the Iliad without any argument to support it, which apparently has not been adopted by later scholars so far as I can ascertain. At least Faesi and Passow (in his Lexicon) return to the earlier understanding of the line as referring to making good by valor the loss of Achilles, an exhortation which the same god utters without any possible ambiguity in xiv. 364-375. This suits better the use of θᾶσσον, and the whole context. The line then means in its connection: "Even if Agamemnon is to blame for the absence of Achilles, it is not for us to be slack in fighting, *but let us at once make up the loss; the courage of the brave is not beyond repair.* But do ye no longer, being all chief heroes in the host, neglect your prerogative of valorous defence." On this view, all lack of reference or place for

reference to book ix. vanishes. The conversation between Achilles and Patroklos in xvi. 21-100 and the address of Patroklos to his troops in xvi. 269-274 are the next passages which Grote takes up, and the difficulty that he finds with all of them is the same, that they are inconsistent with the previous occurrence of such an offer of restitution and compensation as is made in the ninth book. It is not perhaps worth while to consider the passages singly; let it suffice to say that while with Grote's conception of the wrath of Achilles, that it would yield at once to the offer of the ninth book, they are fatal to the genuineness of that book, on the other hand if one reads them with the idea of Achilles's wrath expressed by him in i. and xvi., there appears no reason for reference to a premature and unsuccessful attempt to propitiate him, which, when it was made, he treated as an insult. Moreover, as is well suggested by Bäumlein, there is apparent in xi. and xvi. a gradual giving way of the anger of Achilles, which is best explained by the unrecognized effect upon him of the embassy with its offer of atonement. It appears first in his taking interest enough in the combat to stand on his ship and watch it (xi. 600 f.), then in his sending Patroklos to inquire who the wounded man was (611 f.), then in his sympathetic suggestion, when he returns, that it may be pity for the Greeks that makes him come weeping back (xvi. 17 f.), and finally in his permission to him to go into the battle and the eagerness with which he hurries him off (xvi. 80 f., 125-8). A similar gradual change of feeling in the three speeches of Achilles in book ix. is pointed out by Gladstone in his *Homeric Studies*. "I will go home to-morrow," he says to Ulysses; "We will deliberate to-morrow whether to go home or not," to Phoenix; "I will not fight until Hector threatens my ships," to Aias.

The same thing is true of the eighteenth and nineteenth books which has just been said of the sixteenth. The general absence of reference to the ninth book will not seem strange if one has in mind the conception of the wrath of Achilles now suggested, that it made him scorn offers of apology and atonement until it had seen its satisfaction in a full measure

of calamity to the Greek army. The offer made in the ninth book seemed to him of so little importance when made, and is now so driven out of his mind by the death of his friend, that he does not once think of it. His rejection of that offer was but a part of his whole conduct, consistent with the rest, and what he now repents of is the whole, not any part by itself. And why should Agamemnon, in this critical time of reconciliation, risk provoking Achilles again by referring to his refusal of that offer? He does mention, in his only speech bearing upon the quarrel, that he offers now the same gifts as before (xix. 140 f.); but that passage with three others which expressly refer to the embassy of the ninth book Grote regards as interpolations. These passages may well be defended, but there is no room to do it here.

Two other passages, subsequent to the ninth book and seeming to imply it, ought to be mentioned, to which Grote does not refer. One is xi. 794 f., where Nestor says to Patroklos: "But if Achilles is in dread of some oracle and his mother has given him a warning from Zeus, let him send you in his place." Now there is no previous mention of any warning from Thetis to Achilles, except in his words in ix. 410-16. If it be objected that when Patroklos repeats (xvi. 36 f.) this conjecture of Nestor's, Achilles answers (xvi. 50 f.): "I am not in dread of any oracle, nor has my mother warned me from Zeus," the answer is plain that the denial of Achilles means: "That is not my reason for refusing to go into battle." Such a form of expression in Greek is illustrated often, e. g. Ev. Joan. ix. 3. Again, when in xiv. 74-81 Agamemnon for the third time proposes a return home, none of the chiefs present suggests an effort to appease Achilles, or makes any reference to him. Does not this, in the *Achilleid*, consist perfectly with the previous failure of such an attempt, and need explanation if the ninth book is rejected? The first occasion of such a proposition was in ii. 139 ff., when all the chiefs knew it was not made in earnest; the second in ix. 26 ff., when the embassy follows.

Let me now say in conclusion again that my only attempt has been to present what could fairly be said (passing over a

few minor points) in criticism of Grote's theory and of his arguments in support of it. My study of it has impressed me with admiration of his clear view and firm grasp of the substance of the poem, which is the more remarkable when one remembers that this theory forms but a brief episode in the grand movement of his noble investigation of Greek history. His theory may be open to fewer objections than any other, but I do not see how some of the objections to it can be removed. It will perhaps seem to some that my criticisms upon it are concerned with minute points. It is on single lines and phrases, however, that the question must turn in great measure, as there is no ground for argument except in what the poem itself furnishes. On the whole I would rather accept the *Iliad* as it is, considering it a single poem with a poetic freedom in disregarding inconsistencies and subordinating even natural sequence to occasional impulse, than accept Grote's explanation of its present shape.

It may be worth while to add that, so far as I can ascertain, the different investigations seeking in the language or metre of different parts of the Homeric poems a criterion of difference of age, such as Giseke's on the use of prepositions, the order of words, the use of enclitics, etc., Friedländer's on the unique words (*ἅπαξ εἰρημένα*), do not show any distinction between books ii.-vii. and the rest of the *Iliad*.

III.—*On Negative Commands in Greek.*

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THE object of this paper is not to produce any new facts, but to offer, in a sort of tentative way, an explanation of the phenomena which are stated, perhaps, with sufficient clearness and accuracy in some of the grammars.

The fundamental rule is to employ the present imperative or the aorist subjunctive in prohibitions, while in positive commands the imperative of both tenses is employed. Now, commands are more likely to give offense than any other ordinary communications, and the imperative is the full representative of a command. Hence, even in positive commands, we not unfrequently find substitutes for this mood, such as the optative with *äv*, or even without *äv*, in Greek; the optative in Sanskrit; the future indicative second person and the subjunctive third person in Latin, etc. These substitutes have their origin in another sort of sentence, and do not have the direct and full force of a command. But negative commands are much more likely to give offense than positive ones, because, first, they are more likely to do violence to the will of the person commanded, while, when a positive command is given, the person is often expecting or awaiting orders; and, secondly, they indicate, on the part of the one who commands, an assumption that the one commanded is going to do the thing forbidden unless he is prohibited. Hence, "young America," when told not to do a thing, is often heard to reply: "You'd better wait till I go to do it," or even: "You'd better wait till I do it." But prohibitions, like all commands, are of two kinds: first, those which are general, such as laws, moral admonitions, standing injunctions, etc.; and, secondly, those which are special, relating to some individual act that is about to be performed unless prohibited. Prohibitions of the former class are not so likely to give offense. Generally the order is of such a character that it is expected and approved of, and frequently a person included under it does

not take it to himself at all, but thinks it is more especially adapted to the case of his neighbors, such as : *μὴ κλέπτε*, "Thou shalt not steal." But special commands come directly home to the person addressed, and if he was, otherwise, going to do the thing prohibited, he feels that violence is done to his will, and that the course he was going to pursue is implied to be an improper one ; while, if he was not going to do it, he feels a just indignation at the presumption of the person who forbids it.

Now, in Greek the present imperative is used for the most part in general commands, and hence is retained in negative sentences, while the aorist imperative is employed for special commands, and so needs mollifying when negative. Of course it is not claimed that the present is used *only* for general cases ; but the very fact that this is its normal use renders it a modified command when it is applied to a special case. The general order is issued, and the special case is merely one of its applications, and the shoe need not be worn if it does not fit.

Commands expressed in the third person frequently have reference to absent individuals, and as we are not as considerate towards them as we are towards those present, we should expect to find the aorist imperative retained in such instances ; and in fact, just in the third person, the aorist imperative is by no means uncommon. But why is it not universal ? Because it does not always apply to an absent individual, although it is in the third person, as : *μή σοι μελήσῃ*, 'let it not concern you.' An apparent objection is found in the fact that sometimes we meet with the aorist imperative third person when the command does apply to the person addressed, as PLAT. Crit. 45, B : *μήτε ταῦτα φοβούμενος ἀποκάμῃς σαυτὸν σῶσαι, μήτε ὁ ἔλεγες ἐν τῷ δικάστηρίῳ δυσχερές σοι γένεσθω* : where we have the aorist subjunctive second person and the aorist imperative third person both in one sentence, and it is quite evident that the imperative relates, as well as the subjunctive, to the person addressed. But we must not suppose that the Greeks, after their language was fully developed, were conscious of the reason why they adopted certain modes of expression : why, for instance, they employed the subjunctive rather than the

imperative in negative commands. The cause of this modification did not have universal application in the third person; whence was developed the usage of employing sometimes the one form and sometimes the other, and in the course of time, when the origin of the distinction was lost sight of, they began to use both forms almost or quite indiscriminately. Moreover, the very fact of the commands being expressed, though only grammatically, in the third person, serves as a sort of mollification.

There is, however, one kind of negative commands, not included either in the general or in the special; and that is, when the act forbidden is already going on. In such cases the aorist does not suit, as it looks to an individual act in the immediate future. Strictly and logically, the imperative of the word denoting the act in question should not be employed at all, but the participle with the aorist imperative of *παύομαι*, 'cease,' as: *παῦσαι λαλῶν*, 'cease speaking'; but the one positive command, 'cease' or 'stop' doing a thing, is as offensive as a negative command, and virtually is one. Hence, when we choose to employ the ordinary expression of a command without the circumlocution with *παῦσαι*, the only way is to place it under the form of a general injunction 'not to do' so and so, or the progressive form '*do not continue*' doing so and so, and let the person so commanded *infer* that he is to cease. Consequently we find the present imperative in such cases. It may be added that in this instance the principal element of discourtesy—the assumption that the act is going to be performed—no longer exists, for the act is already going on.

It may be objected to all this that, if it is true, the principle should apply to other languages as well as to Greek. This will depend on two things: first, the sense of politeness and regard for the feelings of others on the part of the people who employ the language, and, secondly, the capability of their inflected forms to furnish a substitute for the imperative. (Of course a substitute would be developed if the people were desirous of being polite or courteous when they were creating their inflections; but the sense of politeness would not be very considerable at so early a period in the history of any people.)

Accordingly, as far as I am acquainted with the ancient inflected languages, the principle holds good everywhere. In Latin, the retention of the long, that is, the *general*, form of the imperative, and the replacement of the short, or *special*, form by the subjunctive in negative commands, are familiar to every one. Of course there are exceptions here, as everywhere else; but some of these are only apparent, as: *Tu ne cede malis sed contra audentior ito*, where *cede* is not used so much where *cesseris* or *cedas* should be expected, as where we should look for *cedito*, as is shown by *ito* connected with it; that is, the short form is used in the sense of the long, and so does not need modifying.

So in Sanskrit, the aorist tense with suppression of the augment is often employed instead of the imperative mood in negative sentences, as *mā krthās* (= *μὴ ποιήσης*). The imperative may also be employed; but to what extent the one is used for special and the other for general prohibitions (if the distinction exists at all), I am not prepared to say; but I have no doubt that such a distinction once existed, just as it once did between the imperfect and the aorist; and it is certain that this substitute for the imperative was introduced to mollify negative commands only.

Even in Hebrew, where there is no distinction between the special and the general, the imperative is never used in prohibitions, whether general or special, but the future is employed, as *לֹא תִגְנוּב* (= *μὴ κλέπτῃς* or *οὐ κλέψεις*).

In modern languages, where inflections have to a considerable extent disappeared, but courtesy has attained a high grade, it has become customary to modify almost all commands addressed to individuals, by some apologetic expression, such as 'please,' 'bitte,' 's'il vous plait'; and in some modern languages the negative imperative is never employed at all.

IV.—*On Hebrew Verb-Etymology.*

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MANY years have elapsed since the first attempts were made to analyze existing Hebrew verbal stems in order, if possible, to reach simpler forms, and yet great uncertainty seems to hang over the results obtained. Such an analysis will be a very valuable contribution to Hebrew lexicography, and must certainly precede any attempt at a comparison between Hebrew and non-Shemitic languages. It is therefore the interest both of the Shemitic student and of the comparative grammarian to engage in the investigation of the form of the Shemitic verb-stem—I say Shemitic, because it is impossible to understand the Hebrew verb or that of any other dialect of the same family, without taking into account all the other dialects, and because their substantial identity is so completely demonstrated as to allow us to use one for the explanation of another. And if the search for Hebrew roots should seem to have anything in common with the search for the Philosopher's Stone, it may be hoped that it will at any rate, like that vain pursuit, bring to light many valuable facts and pave the way to a scientific construction of the verbal system of the language.

A word may be proper as to the relation of the stem to the root. Every word, after all inflectional and other merely modifying elements have been removed, is reduced to a simplest form, which contains the fundamental meaning of the word, and to which the name *root* is commonly given. The significance of this term, however, must always be relative; that is, it must be understood as setting forth the limits of our knowledge with respect to any given phonetic complex, and as being always liable to modification from further research. A set of words called roots by one generation may in the next generation have been resolved into simpler elements, which will then be roots till they are in their turn resolved, and so on. The name *stem* is given to the existing form, which is regarded as resulting from the addition of uninflectional

elements to the root. Thus the Hebrew *katab* 'he wrote' may be conceived of as built up from a form *kat* with some more general meaning, or from a still simpler *ka* having a still more general signification. Or, it may be supposed that *katab* itself is an original form, not developed out of any simpler one. In the latter case the name 'root' will be given to *katab*, in the former *kat* or *ka* will be the 'root' and *katab* the 'stem.' We have here supposed the vowels to be component parts of root or stem; but it may also be inquired whether the original form was not vowelless (*ktb* or *kt*), merely ideal and unpronounceable, the vowels serving to attach some definite formal conception to the word. Further, if *katab* have come from *kat*, it is necessary to inquire into the origin of the added *b*. Here we shall call the existing Hebrew verb-form the stem, and designate by 'root' any simpler form to which the stem may be referred.

The ultimate elements or roots of language are conveniently divided into the two classes, Substantive or Material (Verbs and Nouns) and Relational or Formal (Pronouns and Particles); this division may be adopted without deciding whether it will eventually turn out to be well-founded, that is, without deciding whether the pronominal forms of language have arisen independently of substantive words. The distinction between these two classes is well marked in Hebrew; not so, however, the difference between Verbs and Nouns. A verbal stem, indeed, is in our dictionaries supposed to underlie every nominal form, but strictly speaking every verb is also based on a nominal conception. In the last analysis the grammatical difference between Noun and Verb disappears; the former is the name of a thing considered as an isolated and motionless object of thought, the latter the name of an action or state considered as fused into a unity with its agent or subject. Thus *katab* signifies the act of writing; if the pronoun *na* 'we' or 'us' be added to it, there results *katab-na* 'a writing pertaining to us,' which, viewed as an isolated object, is the noun 'our writing,' but viewed as combined with its agent is the verb 'we write.' And as the Hebrew noun-stem is identical in form with the verb-stem, it is unnecessary to distinguish

between them in an examination into substantive roots. The object of this paper is to give an account of what has been attempted up to this time in the analysis of the Hebrew verbal (that is, substantive or material) stem, so as, if possible, to determine what may be regarded as certain or probable, and what as doubtful or unknown. It will be proper first to inquire as to the present form of the stem, and afterwards as to the possibility of resolving it.

1. It will appear below that the view taken of the form of the existing Shemitic stem is not without influence on the methods of analysis adopted, and it will therefore be necessary to make a somewhat careful statement of the facts. Triliteralism is generally acknowledged to be the distinguishing characteristic of the Shemitic family of languages, that is, a triconsonantal stem, the substantive signification of which resides wholly in the consonants, the function of the vowels being simply to give coloring to the meaning. A few stems contain four letters (quadriliterals) and still fewer contain five (quinqueliterals); the origination of most of these from trilaterals or bilaterals is tolerably obvious. We may therefore take as the type of the ordinary transitive verb the perfect third person singular masculine *katab* (neuter *katib* (*kateb*) and *katub* (*katob*))* . But we may go further and say that no Shemitic verb-stem contains fewer than three consonants, that those that seem to be monosyllabic (namely, the weak verbs, those with second radical doubled, and those with initial, medial, or final weak letters) are really contractions from true trilaterals. The structure of the language would lead us to look with suspicion on this alleged monosyllabism, for the principle of triconsonantism does not exhibit its power wholly or chiefly in fixing the number of stem-letters, but controls the whole word-structure. Namely, it is a consequence of, or rather, a

*Philippi (*Der Grundstamm des starken Verbums im Semitischen*, &c., in the *Morgenländische Forschungen*, *Festschrift Professor Fleischer gewidmet*, Leipzig, 1875, p. 75, sq.) has made it probable that the form of the primitive Shemitic simple stem is not *kataba* or *ktab*, but *katub*. He maintains that the forms in which the personal endings begin with consonants, as *katab-ta* suppose the dissyllabic stem, and that this explains the phenomena of the tone or accent and general vocalization, not only in Hebrew and Arabic, but also in Aramaic.

coördinate fact to, this rigid consonantism, whereby the substantive meaning of the stem is given in its consonants, that the inflectional and other modifications of meaning are largely expressed by the vowels that connect themselves with the stem-consonants, and this in such a way that the significance of the vowel depends on its syllabic position in the word. Hence come certain definite laws of vowel-coloring (by which expression no symbolical significance is claimed for the vowels) and vowel-change, which run through all the word-forms and are obviously based on the triliterality of the stem. Thus, in the large class of segholate nouns the singular is marked by a slender vowel on the first syllable (*malk*, Heb. *melek*), and the plural in Hebrew by a half-vowel in the first syllable and a broad *ā* in the second (*m^llakim*); and this sequence has evidently been observed in the case of monosyllables, where it is obscured by contraction, as in *yōm* 'day' (from *yaum* for *yawm*), plural *yamim* (for *y^wwamim*); in the verb the transitive character is marked by *a*, the intransitive by *i* (*e*) or *u* (*o*) in the second syllable, the passive is denoted by two peculiar vowels (*u* and *i*), and the active and passive participles are distinguished by the vowel-coloring of the second syllable; the diminutive is denoted in Arabic by a uniform sequence of *u* and *ai* (as *rajail* 'a little man' from *rajul* 'a man'), which rigidly maintains itself in all nouns, whatever the number and character of their consonants (*ubaiy* 'little father' from *ab* 'father'); in monosyllabic forms with the second consonant doubled by *dagesh forte*, the reduplication is resolved when the dissyllabic sequence of vowels is necessary to characterize the form, as from the perfect *sab* the participles in Hebrew are *sōbeb*, *sabub*, the Arabic infinitive (Stem III.) is *sibāb*, the Hebrew noun *am* 'people' sometimes simply doubles its second consonant (when a half-vowel occurs in the second syllable), as *ammi* 'my people' (for *am^emi*), sometimes resolves the reduplication (when a full vowel stands in the second place) as plural *amamim*.* As with the phenomena of vowel-color-

* Ewald and others hold that the primitive nominal and verbal forms are distinguished by their vowel-coloring, the former having the vowel in the first syllable (*kab*), the latter in the second (*kab*). But noun and verb both go back to th

ing, so with those of vowel-change—they suppose a trilateral dissyllabic stem. The mutations of vowels depend on the nature of the syllable and the place of the tone or accent. Take, for example, the nouns in Hebrew (which indicates the vowel-mutations more minutely by its vowel-notation than any other Shemitic language), the changes in which may be discovered to a great extent by comparison with the related dialects. The vowels of monosyllabic nouns and the changes connected with inflectional additions point naturally to, and are satisfactorily explained only from dissyllabic stems. *Moth* (for *mauth*) is from *maweth* (*m^aw^at*), *zēth* (for *zaith*) from *zāyeth* (*z^ay^at*), *shem* ‘name’ (Aramaic *shum*) is from a form with final Waw or Yod, as is shown by the Arabic and Aramaic plurals *samaw.at* and *sh^hmah.at*, *yād* ‘hand’ from a similar stem (Arab. plu. *yudiy.yun*), many monosyllables double the second consonant before additions (as *ēm*, *shēn*, *kēn*, *ām*, *yām*, *tal* and others), *ab* ‘father’ and *ah* ‘brother’ have the forms *abi* and *ahi* before suffixes, and the numeral *shēsh* ‘six’ appears in Arabic as *shadsh*. Here the origin of the two first forms lies on the surface, and in the others by comparison of the plural forms we can trace the vowel of the singular. From an original *zayat* with the accent on the first syllable, the second vowel, being untuned, would naturally be degraded and finally disappear or coalesce with the first, whence *zait*, *zēt*; the broad *ē* of *sem* is an extension of *ā* in *samay* or *i* in *simay*, as *ṣēfer* ‘book’ from *ṣafar* or *ṣifer*, while in *yād* the *ā* has passed into *ā*, and similar extensions occur in the reduplicated forms; *āb* and *āh* are for *abaw* (*abay*) and *ahaw* (*ahay*), whence *ahot* ‘sister’ for *ahawat*. Nor do we find an exception to this general principle in those verbal forms that are cited as examples of true Shemitic monosyllabism. In the concave verbs the three ground-forms with middle *ā*, *i*, and *u*—*kān*, *meth*, *bosh*—are readily explained as contractions from *kawan*, *mawith*,* and *bawash*, while *kān* cannot be accounted for from

form *katab*, on which the present nominal and verbal forms are based; see Philippi, *ubi supra*, p. 75.

* Ewald (*Ausführl. Lehrbuch d. Heb. Spr.* § 54) explains *kān* as from *kawan* and *meth* as from *mawith*, holding to the specifically Hebrew form of the ground-stem (*kātab*, *kāteb*) instead of going back to primitive Shemitic form (*katab*, *katib*).

the stem *kūn*. The persons whose suffixes begin with consonants have in Hebrew *kān* (in Arab. *kūn* from the third class, middle *ū*), *math* (Arab. *mith* shortened from *meth*, Aeth. *meth*), *bōsh* or *bōsh* (Arab. *būsh*, Aeth. *bōsh*), as the firm verbs in *ē* and *ō* write *ā* and *ō* or *ō* in these persons (*kabad.ta*, *yakol.ta*). The Hebrew infinitives *kōn* and *kūn* are for *k'wōn*, imperfect *yākun* for *yak.wun*, participles *kān* and *kūn* for *kāwin* and *kawun*. So the derived stems are easily accounted for from the trilateral ground-stem. The Niphal perfect *nākōn* is for *nakwan*, the weak *w* falling out and the *ā* passing into *ō* as in the active Qal participle of the perfect verb (Heb. *koteb* for original *kātīb*); the infinitive *hikkon* for *hikkān* from *hikkawan* for *hin.kawan*. From the original *hakwan* and *hukwan* the Hiphil *hekin* (for *hakin* and this for *hakan*) and the Hophal *hukan* come without difficulty. The forms of the verbs with middle radical doubled also are easily gotten from the trilateral stem. The infinitive *sob* is for *s'bob* (from *sobeb* or *subeb*, Arab. *sabbun*), the imperfect *yāsob* is for *yasbob* or *yasbab* (Arab. passive *yasbub*, indic. *yasubba*, Aeth. *yesbab*, *yesabeb*, Aram. *yissob* or *yissub* for *yassub*), Niphal *nasab* for *nasbab*, imperfect *yissab* for *yissabeb* (Arab. *yansabbu* for *yansabību*), Hiphil *heseb* = *hasab* for *hasbab* (Arab. *asabba* for *asbaba*, Aram. *asseb* for *asbeb*, *asbab*, Aeth. *asbaba*), Hophal *husab* for *husbab*. It is equally clear that the forms of Verbs First Yod and Waw are based on the trilateral stem: Qal imperfect *yesheb* for *yawshab* (the *w* falling out) and *yirash* for *yirash* (Aeth. *ye.wger*, *yelad*) Hiphil *hoshib* for *haushab*, *hawshab* (Aeth. *awlada*), *hetib* for *haitab* = *haytab* (Aeth. *aytaba*) Hophal *hushab* = *huwshab*, Niphal *noshab* for *naushab* = *nawshab*. Evidently the various forms of the verb in the several dialects become symmetrical and regular if we suppose them based on a triconsonantal stem, and some of them cannot be otherwise understood. In the geminated verb the doubling of the second letter before a vowel and the frequent resolution of the doubled letter point to a stem *sabab*. In the concave verb the Qal perfect *kān* cannot be satisfactorily accounted for from a stem *kun*, for if we suppose a *ku.an* contracted into *kan* (as Arab. Pass. *kuwina* into *kina*), this is really intro-

ducing a middle consonant *w*; on the other hand, a stem *kan* will not account for the introduction of the radical *w* in the intensive stems in Arabic, Aethiopic, and Aramaic. The causative forms *hoshib* and *hetib* might be considered as coming from stems with initial vowel, *ushab* and *itab*, but such stems do not actually exist; we find only trilaterals beginning with full consonantal *w* and *y*. Finally, these two letters appear as consonants in the Verbs Third Waw and Yod (Heb. Third He, Aram. Third Aleph), rarely in Hebrew (in Qal pass. participle, and sporadically elsewhere), and regularly in certain forms in Arabic, Aethiopic and Aramaic; and all other forms receive easy explanation on the supposition of a trilateral stem. These three classes of verbs, therefore, cannot be properly designated as having initial, medial, and final vowels.

The quadriliteral of the form *gargar* does not exhibit an existing biliteral, for the *gar* is not found separate. The *gar* is the basis of *gargar* as *sab* is of *sabab*, but neither *sab* nor *gar* is an actual word, nor is any such substantive word to be found in the Shemitic languages without an intimation in its inflections that it is based on a longer form. We are therefore warranted in saying that there does not exist a Shemitic substantive stem of less than three consonants, and that any stem-analysis must consist essentially in the removal of one or more consonants. In an active trilateral the first vowel must always be *a*, and it may be inferred from the preceding statements that this *a* must always be short (in the primitive stem). The second vowel may be *a*, *i*, or *u*, of which the first marks the transitive, the second and third the intransitive character. The quadriliteral has only *a* in both syllables, broadened into *ā* or diminished to *ē* or taking the form *ē*.

2. Having fixed the form of the existing Hebrew stem, we may now go on to examine the attempts that have been made to reduce it to a simpler form. It obviously does not follow that the present trilateralism is original; the question as to the original form can be answered only by an examination of the phenomena of Hebrew and the other Shemitic languages. It has, indeed, been shown that the whole structure of these languages is based on trilateralism, that this, therefore, is an

essential feature of primitive Shemitism, that a Shemitic primitive language without trilateralism cannot be conceived. But while this is undeniable (taking the term Shemitic to indicate the present stage of development) it by no means follows that there may not have been a process of growth in the language, whereby it attained its present form from simpler beginnings. The possibility of such a growth of stems is, however, denied (as by Renan*) on grounds derived from the linguistic truth first clearly set forth by William von Humboldt,† that the genesis and development of a language is to be regarded not as an accidental, mechanical conglomeration of sounds, but as the realization of an idea; that we are not to suppose, for example, that Sanskrit or Arabic is a mere advance from a condition of Chinese monosyllabism to a position that Chinese itself might have reached under favoring circumstances; this would be as unfounded a view as that a fern might under favorable conditions grow into an oak. Impressed with this truth, Renan contends that a language must leap into existence full-grown, that, being the product of the spirit of a people, it could not be originated before that spirit had come to full self-consciousness, and, having been originated, could undergo no change. However, Humboldt's view does not exclude growth, but rather supposes it. He rightly claims for each language a definite "form," which clothes itself with "matter," and manifests itself by the manner in which it selects and employs its matter. While this view insists on the unity and individuality of each tongue, and opposes the opinion that linguistic growth is a merely accidental, aimless progression, it is perfectly consistent with the utmost individual freedom in the word-makers, and indeed assumes such freedom as the instrument and immediate originating cause of a language. It is only by combining these two apparently diverse elements that we can account for Shemitic trilateralism and the phenomena of language in

* *Histoire Générale des Langues Sémitiques*, pp. 97 sq., 418, 421.

† *Ueber die Verschiedenheit des menschlichen Sprachbaues* (Vol. VI. of Kuhn and Schleicher's *Beiträge*). Comp. Steinthal, *Die Sprachwissenschaft W. v. Humboldts*, and *Charakteristik der hauptsächlichsten Typen des Sprachbaues*.

general. Men at first probably get the materials for language from the imitation of natural sounds; yet each group of men living together and learning from one another is controlled by its individual character in the formation of its language, its own nature sets bounds to its freedom of movement, and, on the other hand, within these bounds it is capable of effecting any linguistic changes. Whether any particular phenomenon falls within these limits can only be determined by examination. Humboldt regarded Shemitic original monosyllables as possible and even probable;* properly speaking, if Shemitic stems were resolved into monosyllables, the monosyllabic language thus reached would not be Shemitic, but it would present the materials out of which Shemitism was developed. On the other hand, it is unnecessary to assert that original polysyllabic roots (with or without a simpler ideal basis) are impossible. Though it may seem improbable that the primitive word-makers would give a polysyllabic name to a new object, especially that they should mentally frame a polysyllable on the basis of an ideal monosyllable, yet we know too little of the conditions of primitive language and of the nature of ultimate roots to decide positively such questions as these.† Assuming only the possibility of stem-analysis, let us inquire whether there is reason to hold to the existence of a form simpler than the present triliteral stem. The inquiry will be facilitated by an examination of the various schemes of analysis that have been proposed, and even those that have proved untenable may not be without instruction.

(1). The *reduplication-theory* of Ernst Meier is one of these. Assuming the original biliterality of the roots, and struck by such cases of reduplication as the geminated verb (*sabab*), and by the fact the ground-stem of the verb (*katab*) is a perfect (but in reality proceeding on Indo-European analogies),

* *Ubi supra*, p. 405.

† Without adducing Indo-European analogies to decide a principle of Shemitic growth, we may illustrate now the possibility of the growth of stems from the Indo European multiliteral verbs, in which the longer forms exist alongside of shorter and obviously original ones, as Sanskrit *yug* and *yu*, *mut* and *ma* (Schleicher, *Compendium der Indogermanischen Sprachen*, § 206), English 'stood' (root 'sta'), 'stack,' 'stall,' etc.

Meier concludes that all Hebrew stems are perfects in signification and formed by reduplication (and subsequent vowel-changes) from original biliterals. He supposes three* modes of reduplication: the first radical letter may be repeated at the beginning, as *hakam* from *kam* (*hakam* for *kakam*, *h* interchanging with *k*); or the first letter may be added at the end, as *saras* from *sar* and *nagan* from *nag*; or the second letter may be repeated at the end, as *halal* from *hal*. In the biliteral root, he thinks, the fundamental signification belongs essentially to the final consonant (whereby he would seem to point to a reduction of biliterals to unilaterals), and the reduplicated root-consonant, which forms the stem, has a merely inflectional function, namely, to express the perfect. But, as this modifying consonant undergoes various phonetic changes, he regards these changes as indicating *nuances* in the ground-signification of the root. Thus, if the root *ḥaz* signifies 'cut,' the stem *ḥazaḥ* properly denotes merely the completed action of cutting, but the stems *ḥazāh*, *ḥazar*, *ḥazab*, in which the reduplicated consonant (*z*) is transformed, express modifications of the notion of cutting or peculiar sorts of cutting. In fact, then, Meier does not restrict his reduplicated consonant to an inflectional function, but attributes to it a substantive force, while at the same time he does not distinguish between the effects of the reduplication of the first and of the second letters of the root, though he regards the signification as residing solely in the latter. To all roots whose second letter is a labial he attributes the signification 'bring together,' to all others (those ending in dentals, linguals, palatals, or gutturals) the signification 'separate'; to these two conceptions he traces all the verbs in the language.† That the signification of the root is in the second consonant he undertakes to show by actual examination of the roots; and, to set aside the difficulty that would arise if the two root-consonants were of the same

* He at first assumed a fourth mode, by simple strengthening of the root-vowel, as *kān* from *kan*, but afterwards abandoned this view and regarded *kan* as = *kawān*; see his *Hebräisches Wurzelwörterbuch*, Preface, p. x.

† He reckons the original Hebrew roots to be about twenty-four in number, the collateral roots two hundred and ninety, and the perfect-stems one thousand eight hundred.

organ of speech, he holds that such identity does not occur, any form in which it seems to occur arising invariably from reduplication and subsequent contraction.

Meier has much useful matter in his *Dictionary of Hebrew Verbal Roots*, but the vices of his system of analysis are obvious and striking. The fundamental and decisive objection to it is that the assumed reduplication of the root-letter does not in fact always carry with it the perfect notion of completedness; the infinitive and participle and notably the form called the "imperfect" frequently express the opposite idea, and yet all these forms contain the supposed reduplication. If the notion of completedness inheres in the form *katab* (reduplicated, according to Meier, from *kat*, so that *katat* = *katah* = *katab*), how can it have vanished from the form *yi.ktob*?* Moreover, Meier connects the completedness with past time. "The root," says he, "extends itself, as it were, into the past to indicate the perfect, and by this direction of growth (the reduplicated letter standing regularly at the beginning, that is, *behind*) sets forth very vividly, or, if you will, symbolically the conception of higher antiquity, of the having come into existence, that is, of completion." But this is directly in the teeth of Hebrew usage, according to which the idea of time does not enter into these verb-forms at all, perfect and imperfect being each used to express present, past, and future time; and the symbolic interpretation of the position of letters is, to say the least, very doubtful. Elsewhere in Hebrew the doubling of a letter, where it is clearly significant at all (that is, not merely euphonic) always expresses intensity, and there is no trace in the language of a properly inflectional function assigned to it. It might be anticipated that, in order to establish the fact that no root is composed of letters of the same organ and to account for the diverse forms of the reduplicated letter, it would be necessary to provide great latitude in the interchanges of consonants,

* Meier merely says (*ubi supra*, p. 14) that the true signification of the Imperfect (incompletedness) stands in contrast with the completedness of the Perfect. Comp. Grill in *Z. D. M. G.*, xxvii. 3, p. 440, sq.

and Meier in fact allows interchange without regard to organ;* but his proof of these laws is based on and interwoven into his hypothesis of reduplication, and stands or falls with it. He has himself, in fact, partially abandoned this hypothesis in assigning modifications of meaning to the various forms of his reduplicated letter, and has thus approached a correcter view of stem-analysis. Stripped of its untenable perfect-feature,† his dictionary is a comparison of stems for the purpose of determining which letters are original and which merely modifying; but his method has often led him by wrong paths to incorrect results.

(2). The theory of the *composition* of roots or stems as an explanation of trilaterals has no longer any supporters, but it is still employed to account for some quadrilaterals and quinqueliterals (nouns). It is, however, so contradictory to Shemitic usage that it is not likely to be retained even for these. No example of real composition of substantive words, that is, where components are combined into a true unity of form and meaning, exists in Hebrew. In the nouns that are called compound (chiefly proper names) the components in all cases retain their full form (except the divine name Jahweh, which, from the frequency of its use, suffers abbreviation) and stand in simple syntactical relation to one another: either in a possessive relation (construct state), or in simple apposition, or in copulative relation (like Sanskrit *Dvandva*), or as subject and predicate of a complete sentence, or the first is a negative; in all cases the components are felt to be distinct, there is no such fusion as in the Indo-European languages. The older authors (as Schultens and Schröder)

*As the interchange of gutturals and labials, p. 230. This he supports by citing the Indo-European transition of *k* into *p* through a *kv* (*kp*), without, however, adducing any such intermediary *kp* in Shemitic.

† Meier rightly says (Preface, p. vi. sq.) that no one has yet pointed out what is the mark of completedness in the Hebrew ground-form (*katab*), and that Ewald's distinction of the verbal stem (*ktab*) from the nominal (*katb*) by the place of the vowel, is merely a description, not an explanation. In fact, Ewald's remark, even if correct, would account only for the verbal conception, not for the notion of completedness in the ground-form. The difference of meaning between *katab* and *yikbob* is probably largely due to *usus loquendi*.

allowed themselves much latitude in combining trilaterals diverse in meaning into quadrilaterals with compound significations. From *paras* 'spread out' and *paraz* 'cut' Schröder gets *pirsez* 'spread out by cutting' ($A B C + A B D = A B C D$), from *kabal* 'fold' and *rabal* 'to be loose,' *kirbel* 'to loosen by folding' ($A B C + D B C = A D B C$). Joh. Simonis prefers to combine only synonymous stems, and explains *kirbel* as made up of *karab* and *kabal* 'to bind,' and *ritpes* from *ratab* and *tapas* 'to be sappy.' All such forms are now explained (in accordance with the principle of determination to be referred to below) as arising from the addition or insertion of formative letters. The same explanation may be offered in the case of certain quinqueliterals held by Gêsenius and Ewald* to be true compounds or fusions, as for example, *salanan* (שלאנן), supposed to come from the fusion of *salah* and *saanan*, but more simply explained as made from the latter stem by the insertion of *l* (as Gesenius himself regards it in the *Lehrg.* II. 863). The other quinqueliterals no doubt originated in similar ways. It is not at all probable that *zefardê* (צפרדע) 'a frog' means 'marsh-hopper' or 'marsh-croaker' (literally, 'hop-marsh,' 'croak-marsh'); it is rather a further formation from *zafar* 'to croak.' Practically, indeed, as Gesenius points out (*Lehrg.* II. 867) this process of fusion amounts merely to the insertion of an additional letter or letters into the stem; it is in form nothing but an awkward application of the theory of root-determinatives.

(3). A subdivision of the theory of composition is the *prepositional* hypothesis of Fürst and Franz Delitzsch,† which supposes that trilaterals are made from biliterals by prefixed particles (in the manner of the Indo-European languages) so that we have only to strip away the prepositional *ba*, *da*, or *pa* to obtain the simple biliteral. The decisive objections to this hypothesis are fully stated by Pott, Renan, and Friedrich Delitzsch.‡ It is opposed by the analogy of the Shemitic

* Gesenius, *Lehrgebäude*, II. 866; Ewald, *Heb. Lehrbuch* (7th ed.), p. 278, sq.

† Delitzsch, *Jesurun*; Fürst's *Heb. Concordance*, Preface, p. 8.

‡ Pott, *Etymologische Forschungen*, II. 1. p. 92; Renan, *Hist. des Lang. Sinitiques*, p. 451; Friedrich Delitzsch, *Indogermanisch-Semitische Wurzelverwandschaft*, pp. 6, 7, 69.

languages, which know nothing of true composition, and by the fact that the supposed prepositions have left no discernible trace in the signification of the verbs, are not now to be found in separate form in the language, and (as against the particular view of the authors) could not have existed in composition in a presumed primitive Shemitic-Indo-European stage of language. This hypothesis owed its birth to a determination to effect a comparison between the Shemitic and Indo-European families of languages, and is guided in its details by Indo-European analogies. It is silently dropped by Fürst in his Hebrew Lexicon, and is probably no longer held by Delitzsch.*

(4). The unsatisfactoriness of the hypotheses above described has gradually led Shemitic scholars to the conviction that the resolution of triliteral stems must be effected, if at all, by a theory of *affixes* (prefixes, interfixes, suffixes), formative letters with substantive, uninflectional powers. Among the phenomena of the Shemitic languages the derived stems seemed, indeed, to suggest a mode by which triliterals might have come from biliterals as quadriliterals (Hiphil, Niphal, etc.) and quinqueliterals (Pealal, etc.) have sprung from triliterals (so De Lagarde). These are indeed examples of stems formed by consonantal additions (initial, medial, and final) to other stems, but they are merely relational modifications of the

* The same theory of composition is held by Mr. F. J. Crawford in his *Horae Hebraicae*, London, 1868, and by M. E. de Campos-Leyza in his *Clef de l'Interprétation Hébraïque*, Bordeaux, 1872. These books show commendable industry and zeal, but unhappily exhibit a total lack of sympathy with the sound method of modern scientific etymology. One of them (that of Mr. Crawford) relies largely on Keltic, the other (that of M. Campos-Leyza) largely on Spanish to explain Hebrew words. Mr. Crawford even offers emendations of the text of the Psalms and other books of the Bible from the Irish and the Persian. Their etymological analyses and combinations, especially their free use of particles in composition are perfectly arbitrary and fanciful, and, notwithstanding the good intention of the authors, can serve only to confuse our knowledge of the language. Mr. Wale's *Book of Hebrew Roots*, London, 1876, deserves the severest condemnation as an utter misrepresentation of Hebrew under the form of a contribution to biblical studies. Its wild biblical exegesis is mixed with such etymologies as the following: *Ab* 'father' he holds to be composed of *aleph* 'the first' and *beth* 'in,' the father being 'the first in' the house in dignity, while *bar* 'son' is *beth* + *resh* 'in the head,' that is, 'in the father'; *Abba* he calls a "junction of Syriac and Greek"! Such a book is a disgrace to England and to the Nineteenth Century.

ground-stem (expressing intensity, causation, reflectivity, reciprocity, and the like), and are so far wholly unlike the trilaterals, in which the added element must be supposed to have a substantive or material force. Besides, while in the derived stems the formative letters employed are comparatively few (*n, t, s*, and perhaps *y, w, h*), all the letters of the alphabet seem to be used in the formation of trilaterals from bilaterals. And then the derived stems could not have originated till after the trilateral scheme of stems was fully elaborated by the language, since which time we have examples of quadrilaterals formed by substantive additions to trilaterals with a wholly different sense from that of the quadrilateral derived stems; from which it seems clear that the linguistic conceptions in the two classes are entirely different. Somewhat similar to this is the *nominal* theory of Ascoli, which supposes that the existing trilaterals and quadrilaterals are denominatives made from bilaterals and trilaterals by the addition of nominal terminations. But, though denominatives are not rare in the Shemitic languages, it is fatal to Ascoli's view that his supposed nominal terminations cannot be shown to exist now in the language, and that the verbs held to be thus formed do not show a corresponding modification of signification.

We are thus led to the opinion that the formative additions to which the trilaterals owe their origin are not inflectional, and to seek some other account of them. That such formative additions do exist in the trilaterals has long been believed. Jewish lexicographers of the twelfth century reduced all Hebrew verbs to bilaterals, and early Arabic grammarians recognized groups of connected stems in which a common signification was attached to two letters. Forster in the sixteenth century endeavored to fix the meanings of Hebrew words by comparing those that had like consonants, and Neumann in the succeeding century derived the significations of words from the onomatopoeic signification of the separate letters.* J. D. Michaelis, Adelung, and W. von Humboldt speak of the great improbability of dissyllabic roots. The

* Gesen. *Lehrg.* i. 184. In his *Heb. Dictionary* Gesenius also confines himself almost entirely to onomatopoeia in his stem-analysis.

numerous groups of stems related in form and meaning point to the obvious and now generally recognized fact of a common biliteral element in them, and it may be regarded as established that all Shemitic substantive stems (except the few multilaterals) are trilaterals resting on biliterals.

Here it may be remarked that for the purposes of the lexicographer it is immaterial whether this primitive basal biliteral be regarded as having had a pronounceable form and a separate historical existence, or as having been a merely ideal, unpronounceable root on which the stems were mentally formed. The root has no separate existence for us; we have to do merely with the stem and can determine its meaning equally well from an ideal as from an objectively actual root. When we compare the group: *ḵatal* 'kill,' *ḵataf* 'pluck,' *ḵazab* 'shear,' *ḵazah* 'destroy,' 'decide,' *ḵazaf* 'cut in pieces' (Arab.), *ḵazaz* 'trim,' all that we need is to determine the root that underlies these stems, its signification, and the modifications of meaning effected by the various formative additions. If we are warranted in holding that the root is *ḵaz* or *ḵat* and that it means 'cut,' then we may regard *ḵatal* as = *ḵat* + *l*, and may proceed to inquire into the force and origin of the *l* wholly without respect to the question whether there was ever any such actually existent word as *ḵat*. But while the lexicographer need not concern himself with this question, it has some interest for the student of language, who desires to reproduce as far as possible the beginnings of speech. Why should the primitive Shemites not have actually spoken these monosyllabic roots? It is not sufficient to say, as Renan docs, that the transition from the biliteral to the trilateral is inconceivable. It is difficult, perhaps, but not more difficult than the supposition of a people habitually framing dissyllables on ideally conceived monosyllables. Or, if it is said that a Shemitic root is a vowelless consonant-complex (as *kt*) expressing an abstract idea without relations to other things, and that it ceases to be a root as soon as it is provided with a vowel, and thus assumes a particular relation to other things*—it may be replied that it is quite possible to go

* See Dillmann, *Aeth. Gram.* p. 91 sq., who, after having said that there are "no true substantive roots in the existing language," somewhat inconsistently

back to the ante-inflectional stage of the language and suppose a word *kat* expressing the idea of 'cutting' without relation to any person or thing, and without variation of vowel. Such words might easily form the materials of a spoken language (as is substantially the case in the Chinese now), and might then by some process be enlarged into triliterals. There is no satisfactory reason why we should not suppose that the roots were actually pronounced, and indeed, as Philippi* remarks, we seem to have in the quadriliteral of the form *gargar* an actual primitive monosyllable, consonants and vowel. It is possible that there was no difference of vowels in the primitive roots, in which case the vowel would be *ā*.† In any case the form *kat* is the basis of all existing triliterals of the forms *katab*, *katib*, *katub*.

The first attempts made in the present century to account for the development of *katab* out of *kat* proceeded in a purely mechanical way; it was supposed that the third consonant was the hardening of a root-vowel, this process being first applied in the case of the weak stems and then extended by analogy to the others. Gesenius in 1817 suggested (*Lehrg.*, i. 185) that the introduction of this law of trilateralism was synchronous with the introduction of Shemitic writing, the vowel-letters (consonants) having been first employed to indicate the vowel-sounds, and then endowed with independent consonantal power. Thus the triliterals with weak initial, medial, and final letters might have originated, and by grammatical reflection the literary leaders in early times, the priests, might have made the law universal. Though the alphabetic side of this hypothesis, which was based on a misconception of the historical origin of Shemitic writing,‡

adds that "the *only easily* pronounceable roots are those that have a vowel for their second sound, as *mut*."

* *Ubi supra*, p. 96.

† Comp. Grill in *Z. D. M. G.*, xxvii. 3, p. 449.

‡ Gesenius supposed the Shemitic writing to be Aramaic and Babylonian in origin. In fact no Shemitic people, as far as is now known, has originated an alphabet, and trilateralism was already developed at a time when there is no reason to suppose that writing was practiced by the Shemitic race. Or, if they had writing, we know nothing of its nature. Fürst seems to proceed on this alphabetical view in his *Lehrg. d. Aram. Spr.*, § 98.

disappeared before a better acquaintance with the facts, the mechanical-phonetic side has been adopted and maintained so far as regards the weak stems by many later scholars, as Fürst and Dillmann. According to this view the weak stem-letters are nothing but the consonantization of original root-vowels initial, medial, and final.* Original roots are supposed to be of the forms *kat*, *kāt*, *kīt*, *kūt*, *akat*, *ikat*, *ukat*, whence come *katat*, *ka.at*, *kahat*, *kayat*, *kawat*, *hakat*, *yakat*, *wakat*, *katah*, *katay*, *kataw*, and, since the vowels may also pass into liquids, dentals, and sibilants, *karat* (by inserting *r*), *nakat*, *sakat*, *zakat*, *takat*.

This view supposes a scheme of radical vowels that fails to account for the phenomena of the existing language, and is in contradiction to its analogies. It has already been shown that the present forms of the concave or Middle Waw Verb cannot be satisfactorily explained as coming from a root of the form *kun*. But, if *kān* is from *kū.an* (as Arab. Pass. *kīn* from *kūwin*?) and *sār* from *sī.ar*, we expect *gāl* from *gā.al*, instead of which, however, is found always *gāl*.† The Jewish-Aramaic *āl* (אל) adduced by Fürst (*Lehrg.*, p. 87) is simply the full writing of the ordinary form (ל) as if from a Middle Waw Verb, and not a mere collateral orthographic form of *āl* (from לל). The assumed initial vowel in other roots is opposed to the Shemitic usage according to which every syllable begins with a consonant. If we may suppose that this principle existed in the primitive language before the development of the trilateral stems, it will be impossible to accept a root of the form *akat*, *ikat* or *ukat* (*akt*, *ikt*, *ukt*). A *trā* might be supposed to come from *tar* by metathesis of the vowel, if we could suppose the Aramaic *tra* (תר) to be an original form; but, as we have seen above, *tra* must be considered a diminished form of an original *tara*, as other cases of double initial consonant are to be similarly explained. The existing stems with weak letter in the third place are: Arab. *kataw*, *katay*, *katiy*, *kata'*, Aram. *ktay*, *ktiy*, *кта'*. These forms cannot be explained as from roots with

* Fürst, *ubi supra*, p. 80, sqq; Dillmann, *Aeth. Gram.* pp. 102-107.

† Philippi, *ubi supra*, p. 90.

broad vowels, *kāt* (*ktā*), *kī* (*ktī*), *kūt* (*ktū*), since we should then expect, for example, Arabic *katā'* from *ktā*, whereas the actual form is *kata'*; and if the root be assumed to be *kāt* with short vowel, the supposition that *kataw*, for instance, (כַּתַּו) owes its origin to the addition of a vowel is an unsupported assumption,* since the phenomena may all be explained by supposing the final letter to be a consonant. The phonetic theory, therefore, does not account for the phenomena of the weak stems, and does not offer any explanation of this supposed phonetic expansion except an unexplained tendency to trilitrality in the Shemitic family of languages. We should have to imagine a phonetic euphonic impulse towards triconsonantism that embodied itself first in the resolution and hardening of vowels, and then, having thus created a type for the stems, extended itself so as to employ all consonants as affixes. Such a view is too vague to be accepted as an explanation of the present form of Shemitic verbal stems. A disposition towards consonantal extension is no doubt to be assumed, since that is what has actually taken place; but the impulse to such extension is rather to be found in some felt need of further material for the expression of ideas.

If, then, it may be regarded as altogether probable that all existing Shemitic verbal stems have sprung from monosyllabic biliterals, but not by resolution of root-vowels or by the addition of inflectional elements, it follows that in every trilateral there is one letter that may be regarded as substantively formative, as modifying the signification of the root by its substantive force. The fixing of the value of this modifying letter or *root-determinative*† is the first task of the Hebrew etymologist, and it can be accomplished only by the widest comparison of groups of related stems. In such comparisons several questions arise, on the answers to which will depend the method and results of the investigation.

* Comp. Grill, *ubi supra*, p. 431.

† This term is proposed by Cartius in his *Grundzüge d. Griech. Etymologie*, p. 70, and adopted by Delitzsch and Philippi. It appropriately implies that the letter it designates defines the root, fixes its boundaries, and so converts it into a stem and a word.

In the first place, is there ground for making any distinction between weak and firm letters in their capacity to act as root-determinatives and in their modifying power, or are they to be regarded as equally endowed with modifying force? It has been shown above that the weak letters are not mere phonetic expansions, but true consonantal additions to the root, and therefore presumably significant. Examination of groups of stems differing from one another by a weak letter shows that the modifying effect is as great in such cases as where the determinative is firm. In the groups formed on *kaṭ* and *kaẓ*, as *kaṭaṭ* 'fail,' *kaṭal* 'kill,' *kaṭon* 'be small,' *kaṭaf* 'pluck,' *kaẓab* 'shear,' *kaẓah* 'decide,' Piel 'diminish,' *kaẓa*, Hiphil 'scrape,' *kaẓaz* 'trim,' there is as much distinctness and differentiation in the weak stems as in the others. The groups formed on *sar* (Arabic), as *saraha* 'to send or go away free,' *sarua* 'to go rapidly,' *sartaa* 'to run rapidly,' *sara* (*sarawa*) 'to remove, lay away,' *sara* (*saraya*) 'to make a journey by night,' *sāra* 'to go up or against,' *sara* (Mid. Ya) 'to depart,' *šarafa* 'to turn, repel,' *šāra* 'to turn, separate, collect,' show equal individuality in the weak letters. This is so generally the case that we may conclude that the weak consonants are as really determinative as the firm. Some groups of stems, it is true, differing only by weak letters, show little or no difference of meaning in a given dialect, but this is true of firm stems also; commonly the other dialects in such cases show marked differences between the stems. It is also to be borne in mind that a weak letter may be the diminished form of an original firm letter, and in general that weak letters may be radical as well as determinative, as is obviously the case in doubly and triply weak stems, as *hayah*, *awah*.

In respect to the *position* of the determinative there can be no doubt that the weak determinatives (Aleph, He, Ayin, Waw, Yod) may stand in any place, as in the stems *šar* (*sawar*), Arabic *sara* (*sarawa*), *ašaf*, *yašaf*, *šafah*, *baar*, *bar*, *bara*, *barah*, and that *n* may be initial or final, as in *nabal*, *abal*, *balah* (root *bal*), *ṭaman*, *haṭam* (root *ṭam*). Whether the firm letters may be initial and medial is to be determined

from examination of the stems, and is yet an open question. The liquids *l* and *r* are inserted after the first radical of a trilateral to form a quadrilateral, as *sarbit* from *sebet*, and Gesenius* compares such forms as the synonymous *daras* and *dus* (*dawas*) as showing a softening of the liquid into a vowel; but it is possible that we have here two distinct roots *dar* and *das*, as in *darak* (from *dar*) = *dakak* (from *dak*). Whether *s* and *t* prefixed to trilaterals to form quadrilaterals are root-determinatives or inflectional prefixes is not always clear, and it must be left to further investigation to determine whether these and other firm letters are ever prefixed to bilaterals in order to form trilaterals.

The laws of interchange of consonants have not yet been determined with exactness, and yet it is obvious that this must be done before the widest comparison of stems and roots can be undertaken. Interchange between letters of the same organ of speech may be safely assumed, and, on the other hand, the entirely unrestricted interchange that is employed by some writers is clearly unwarranted,† but between these points there is still room for much arbitrariness. The investigation of the laws of letter-interchange must go hand in hand with the investigation of the fundamental significations of stems, in order in this way to discover the form and meaning of the roots. It is too obvious to need mentioning, that the meanings of stems must be sought by the widest possible comparison in all the Shemitic languages; yet we find that often resort is had to some one language (most commonly the Arabic) that is supposed to preserve the original meaning. When we consider the imperfections of our lexicons, the numerous gradations in the forms and meanings of the stems (so that different groups sometimes seem to run into one another, one giving one root and another

* Heb. Gram., § 30.

† Meier's license in this respect has already been referred to. Another example of similar license is found in Dr. Davies's Heb. Lexicon, in other respects a very judicious and useful manual. He overlooks the fact that the occurrence of two letters in the third place in two synonymous trilaterals (as *katab*, *katal*) does not prove interchange between them, since they may be independent root-determinatives.

a different one for the same stem) and the possible changes in the past that the stems and roots have undergone, we can hardly feel ourselves justified in saying that "all Hebrew roots may be clearly determined and their significations fixed with precision,"* though such a result would undoubtedly throw light on the meaning of the Hebrew writings. Perhaps the most pressing need of Hebrew etymology at this time is an investigation of the significations of the stems.

A few classes of stems indicate their roots by their form : (1) quadrilaterals of the form *gal.gal*, in which there is almost certainly a reduplication of the biliteral ; (2) trilaterals of the form *gal.al*, made by reduplication of the second root-letter, in which we can in many cases perceive a notion of intensity, as in Arabic *darra* 'to emit copiously, go rapidly or abundantly,' Hebrew *gazaz*, Niph. 'to be cut off entirely, extirpated,' *hasar*, Niph. 'to be dried up,' *daḳaḳ* 'to pound to pieces,' *hataṭ* 'to be thoroughly dismayed,' *ḳalal* 'to be slight', Piel 'to curse, blaspheme,' *talal*, Hiph. 'to deceive,' Aeth. *nababa* 'to speak much, be loquacious,' Syr. *hēnan* 'to pity, pardon,' *hēmam* 'to burn with anger or lust.' The idea of intensity seems to be attained here by the addition of a substantive element of the root, while in the intensive derived stems (as Piel) the same result is produced by the phonetic process of reduplication of a letter. Of these two classes (*palal* and *palpal*) there are in Hebrew about one hundred and fifty stems, including nouns ; (3) trilaterals of the form *palap* with first and third letters the same, which probably come from quadrilaterals of the form *palpal* by dropping the fourth letter (as *saras* from *sar.sar*), and trilaterals of the form *papal* out of *palpal* (as *babal* from *bal.bal*) ;† (4) verbs in which two firm letters are combined with medial Waw, Yod, Aleph, or He or initial Aleph, the root in such cases being found in the firm letters.

Other stems (with firm letters, or with initial or final Nun, He, and probably Waw, Yod, or with medial and final Ayin) do not indicate their roots by their form ; they must in each case be subjected to special examination.

* Furst, *Handwörterbuch*, Preface, p. viii.

† In Aethiopic these forms all have the second letter doubled ; see Dillmann's *Aeth. Gram.*, p. 108.

So little has been done towards fixing the meaning and origin of the root-determinatives that it is unnecessary to say more than that recent writers are disposed to find in them pronominal forms. Their origin must for a long time yet remain a mystery. The contribution of the etymologist to the lexicographer will not, indeed, be complete till he shall have fixed the meanings not only of the primitive bilaterals but also of the modifying additions, and we are yet far from this point. But Shemitic etymology, while its results are still comparatively meagre, is now, we may hope, on the right path, and needs only time to rival its sister science of Indo-European etymology in the extent and accuracy of its achievements. In order that this may be accomplished the Shemitic dialects must be thoroughly studied and the laws of their stem-formation determined, and then the nearest lying group of languages (the Egyptian), after a like office has been performed for it, may be brought into comparison with the Shemitic, in order thus gradually to approach nearer to the original forms of these tongues, and so to bring them into relation, if possible, with more remote families of languages.

V.—A Botanico-Philological Problem.

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A noted writer on language, F. Max Müller, in the course of his researches among the facts summarized in Grimm's Law, brought to light, some years ago, a curious parallelism between certain botanical and certain philological phenomena.

He first observed that the Germanic words *bōka*, *buche*, *beech*, and their like, agree in meaning as well as in form with the Latin *fāgus*; while, on the other hand, the same word in Greek, *φῆγος*, signifies 'oak.' He then further found that our *fir*—and the German *föhre* (O. H. G. *foraha*), which has the same sense—is to be regarded as historically identical with the Latin *quercus*, 'oak.' Here, accordingly, if the Greek value of the former word, and the Latin of the latter, be assumed to be the more original, there are evident signs of a transfer of meaning in certain European vocables, from 'oak' to 'beech' and from 'fir' to 'oak,' respectively. This is the philological side.

With these facts in his mind, the same scholar, turning over Lyell's "Antiquity of Man," chanced upon a passage which showed that in the lowest strata of certain peat-bogs in the Danish islands the remains of tree-growth are prevalently fir, while in the central strata they are oak, and in the upper strata beech; the whole region showing now, and having shown ever since the Roman conquest, such a marked proclivity to beeches that oaks are uncommon, and firs can hardly be made to grow there. Thus there has been, clearly and incontestably, a change in the arborescent vegetation of the region, from fir to oak, and again from oak to beech. This is the botanical side.

The analogy between the two sets of facts thus stated is a palpable one. After setting them forth, then (Lectures on Language, American edition, ii. 238 *et seq.*), Mr. Müller proceeds to intimate an actual historical connection between them: the "changes of meaning," he thinks, may have been

“as the shadows cast on language by passing events.” This poetical and somewhat ambiguous phrase he later puts into more definite shape: “The Aryan tribes, all speaking dialects of one and the same language, who came to settle in Europe during the fir period, or the stone age, would naturally have known the fir-tree only;” and the old name of the fir could not well have been changed to mean ‘oak’ “unless the dialect to which it belonged had been living at a time when the fir vegetation was gradually replaced by an oak vegetation” (pp. 250-1). And again: “I venture to suggest that Teutonic and Italic Aryans witnessed the transition of the oak period into the beech period, of the bronze age into the iron age, and that while the Greeks retained *phēgos* in its original sense, the Teutonic and Italian colonists transferred the name, as an appellative, to the new forests that were springing up in their wild homes” (p. 252).

This is Müller’s theory, in its simple outlines. As to the cautions and reservations which he hangs about it, and the (supposed) analogous facts which he brings up from other quarters in its support, we have for the moment nothing to do with them; we will rather turn our attention first to the acceptableness of the hypothesis considered in itself, taken on its own merits.

In the first place, there seem to be *a priori* difficulties in the way of establishing a cause-and-effect relation between the botanical changes and the linguistic. It is sufficiently obvious, and distinctly pointed out by Müller’s quoted authorities, that a complete change of the prevailing tree-growth, from one species to another, would necessarily require many centuries for its accomplishment. There would be, for example, a number of successive generations in whose sight the oaks would be slowly gaining on the firs; other generations before whom the two would be about equally numerous; and yet other generations which would witness the victory of the oak and almost extinction of the fir. How, in this process, should it ever come about that the name of the tree originally prevalent should come to be applied to the tree finally prevalent? If the oak had no name of its own at the outset, during

its period of rarity and inconspicuousness, it would gain one, alongside the fir, as it rose to rivalry with the latter; and if, as the fir was reduced to unimportance, any name died out, it would naturally be the old name of the fir, and not that of the conquering oak. The fact that, in times long out of memory, the fir had been predominant and had borne a certain title, would not furnish the dimmest shadow of a ground for giving the same title to the oak, in the day when it predominated. The probability is so overwhelmingly against any such transfer, that we have a right to refuse except to the most direct and cogent evidence our acceptance of a theory implying a causative connection between the supplanting of one tree by another and the conversion of the name of the former into an appellation for the latter: there are many ways in which words arrive at new meanings, but this certainly is not one of them.

If, indeed, the people who witnessed the double process of supplanting never had any specific names for different kinds of tree, but only one word for 'tree' in general, this word would of course have been applied to the fir alone in the period of exclusive fir-growth; in that of mingled firs and oaks, it would have belonged to both; when the oaks reigned alone, it would have designated only the oak—and so on: becoming finally the title of the beech, when that tree had come to the throne and exterminated all its rivals. But the linguistic facts are far enough from being what this theory would demand. To satisfy its requirements, we ought to find the Latin and Germanic peoples in possession of only a single tree-name. And this name should not only have changed its meaning from 'oak' to 'beech,' but it should show signs of having, at a yet earlier period, signified 'fir.' Unfortunately, that is not the case. In the great mass of the Germanic dialects, for example, it is only the 'oak'-name that has been changed into a 'beech'-name; they still possess the word which Müller assumes to have meant 'fir' from the beginning, and they still use it to mean 'fir.' How is this to be explained? Shall we say that the Germanic tribes in general did not witness the supplanting of the firs by the oaks, but only that

of the oaks by the beeches, reaching Europe after the former part of the process had been accomplished? But if it had been accomplished, and so effectually that the people who watched it as it went on had been compelled to turn their superfluous 'fir'-name into an 'oak'-name, these Germans had no business to bring in the old 'fir'-name, and cling to it so obstinately, since they could have nothing to which they should apply it.

Or take, again, the case of the Italians. They (with a small and more questionable body of Germans) constitute the main-stay of Mr. Müller's theory; for they have both changed their 'fir'-name into an 'oak'-name (*quercus*), and their 'oak'-name into a 'beech'-name (*fāgus*); if anybody in the world sat by and saw the whole drama of transmutation, in both its acts, they are certainly the men. But, judging from their linguistic effect, the two acts must have been going on at the same time, independently, and each with a permanent result; for the change of meaning has taken place in two different words, and both are left in the language. If there were not firs and oaks together in primitive Europe, how should these "Aryan tribes, all speaking dialects of the same language," have had both the original 'fir'-name *quercus*, which they should proceed to convert into a name for 'oak,' and the original 'oak'-name *fāgus*, which they should proceed to convert into a name for 'beech'? If the meaning of 'oak' came into *quercus* because the oaks had come in and crowded out the firs, why did not the meaning of 'beech' afterward come into it, when the oaks in their turn gave way to the beeches? If the meaning of 'beech' came into *fāgus* because the beeches had come in and crowded out the oaks, why should not the meaning of 'oak' have come into it in a similar manner, as a result of the displacement of the firs by the oaks? And why should these two corresponding processes have gone on after such a different fashion, the one of them stopping in the middle, and the other, so far as our knowledge goes, beginning at the middle?

There are other equally puzzling questions suggested by the attempt to reconcile and combine, according to Müller's

theory, the two bodies of facts with which we are dealing; but it is needless to ask them; for we are already in a snarl of difficulties which there seems to be but one way of unraveling. It is this. There was not, as Müller supposes, a double process of displacement and substitution; there were, rather, two independent processes of actual conversion, or transmutation, or transubstantiation. The two did, indeed, take place successively in the Danish isles, and this has led to Müller's slight misapprehension. But in most of Germany only one of them occurred; the firs remained firs, while the oaks were changed into beeches—with an accompanying conversion of the name *φηγός*, 'oak,' into *fāgus*, 'beech.' In Italy, again, both processes went on to their complete result, and simultaneously: all the firs turned into oaks, and all the original oaks turned into beeches—each species, notwithstanding its changed identity, retaining its old name. If Müller can bring himself to accept this slight modification of his ingenious theory, then the second principal class of obstacles in the way of its success will have been surmounted.

If, however, quitting this line of examination, we try another, we shall encounter another line of obstacles, not less formidable: probably it has already risen before the minds of many or of all who have attended to this exposition. Is it indeed true that the Danish peat-bogs are to be taken as furnishing in their successive layers decisive indications respecting the history of arborescent vegetation through the whole Germanic and Italic territory—not to speak of the rest of Europe? Have the oaks, as Mr. Müller appears to assume, everywhere driven out the firs? and have they been in their turn replaced by the beeches? Mr. Lyell says: "In the time of the Romans, the Danish isles were covered, as now, with magnificent beech-forests," almost to the exclusion of any other tree-growth: is this the case with all the European territory, except Greece, occupied by our family of languages?

Doubtless there are many who will answer this question promptly and confidently in the negative, and who will even proceed to moralize on the theory which has called it forth. Here, they will admiringly say, we have one of those ideal

philologists sometimes read of in story, but rarely met with in actual life: men of the closet, who are so absorbedly devoted to books and words that these have become to them the sole realities; who never lift their eyes to the nature which surrounds them; who care only for the distinctions of the vocabulary kingdom, and are blind to those of the animal, the vegetable, and the mineral. Müller is said to have grown up to adolescence in Germany, and he has probably at least travelled in Italy; yet, when he finds that *bōka* and *fāgus* have in these countries changed their meaning from 'oak' to 'beech,' he is at once convinced that the German and Italian oaks are, and of right ought to be, beeches. And these persons will probably be confirmed in their view of his personality by the way in which he expresses his willingness to put out the question to be decided by a competent scientific tribunal. "I must leave it," he says (p. 252), "to the geologist and botanist to determine . . . whether the changes of vegetation, as described above, took place in the same rotation over the whole of Europe, or in the North only." It will be seen that, not trusting his own eyes, he is also very particular as to whose eyes shall be allowed to settle the question. Indeed, I have myself personally experienced that, to my cost. Being, as I fondly imagined, a little bit of a "geologist and botanist" in my own right, I ventured, in criticising Müller's theory eleven years ago, to offer my scientific testimony in opposition to it. I said that, instead of firs and oaks and beeches having supplanted and succeeded each other through the whole region occupied by the Germanic and Italic races, "we find all of them, or two of them, still growing peaceably together in many countries."* But Müller, in a reply recently made to my criticism,† being apparently unable to comprehend how one who concerns himself especially with language should presume to know anything on other subjects, fails to see that what I said was meant as testimony, and understands me as simply echoing his suggestion that a scientific oracle, if formally installed and duly inquired at,

* *Oriental and Linguistic Studies*, i. 257.

† *Chips from a German Workshop* (American edition), iv. 302.

might settle the disputed matter. So he quells me with the remark: "Here Professor Whitney is, as usual, ploughing with my heifer;" and then, quoting his former words upon the subject, he goes on to say: "I had consulted several of my own geological friends, and they all told me that there was, as yet, no evidence in Central Europe and Italy of a succession of vegetation different from that in the North, and that, in the present state of geological science, they could say no more. In the absence of evidence to the contrary, I said, Let us wait and see; Professor Whitney says, Don't wait."

Yes, certainly I say, "Don't wait"; a single moment's unnecessary delay is to be deprecated. How can we sit down and fold our hands over a question that affects the weal and woe of so large and respectable a part of mankind? If Central Europe and Italy are really covered, exclusively or prevailingly, with "magnificent beech-forests," then there are a great many millions of people, there and elsewhere, whose mental delusions render it unsafe to trust them any longer outside of an insane asylum. In order to do what I can toward determining their condition and fate, I will follow Müller's own example. He has gathered a set of twenty "principal bones of contention" between himself and me, and challenged me to summon a commission of learned professors to deal with them. I will add to the heap one more bone (bigger and more solid than most of those raked together by him), as follows:

21. Whether Central Europe and Italy are covered with beech-forests, to the exclusion, almost or quite, of other trees, especially of oaks and firs.

And I appeal to him, in my turn, "to choose from among his best friends three who are *professores ordinarii* [of natural science] in any university of England, France, Germany, or Italy; and by their verdict I promise to abide" (Chips, iv. 528). I do not feel willing to accept the outcome of his private and confidential consultations with his "own geological friends," as reported by himself, because it has appeared more than once, in connection with other subjects, that those consultations do not yield unexceptionable results. He declares himself (Chips, iv. 498-9) to have been guided in all his

phonetic investigations, and controlled in their conclusions, by the advice and approval of the highest authorities in physiology and acoustics; yet the influence of Helmholtz and Ellis could not prevent his declaring his independent opinion (Lectures, 6th [English] edition, ii. 133) that the "neutral vowel," the *u* of *but* or *up*, is a non-sonant or whispered element; even as the aid of Main and Hinds could not keep him, in his astronomical reasonings, from assuming (preface to *Rig-Veda*, vol. iv., p. lii.) that, to any given observer, the ecliptic is identical with his own horizon.

It ought to be added, perhaps, that there seems to be another method in which this particular "bone of contention" can be ground up and gotten completely out of the way, without summoning an International High-Joint Commission to chew upon it; and we have reason to wonder that that method did not suggest itself to Müller: for it is a linguistic one. We cannot question that he is familiar with the principle, generally accepted among philologists, that the presence in a language of a certain name implies the presence in the minds of the people speaking that language of some knowledge respecting the thing named; indeed, he himself, in the course of his illustrations, frequently applies or implies it. If we could, for instance, catch an untaught and untravelled savage off one of those Danish islands, with their exclusive growth of magnificent beeches, we should find that he had no names for 'oak' and 'fir,' any more than for 'mahogany' and 'palm.' With this test in our minds, let us examine the various Germanic and Italic dialects; if they contain words for 'fir' and for 'oak,' as well as for 'beech,' then we, as linguists, shall have the right to hold, and to maintain before all the world, that in Central Europe and Italy the beeches have not crowded out the oaks, which had before crowded out the firs. And this, even though the decision of the scientific triumvirate should be adverse to us. For if, in the very passage under discussion, Müller teaches us (Lectures, ii. 252), in case of an apparent discordance between linguistic and craniological evidences, to "protest that the Science of Language has nothing to do with skulls," we should be

justified *à fortiori* in maintaining that she is above having anything to do with trees. As for the presence in the same European tongue of words for both 'fir' and 'oak,' besides 'beech,' we have already above seen some of the facts bearing upon the matter; it may be commended to Müller for a more careful and exhaustive examination, with the hope that we shall find the results in a later edition either of the "Lectures" or of the "Chips."

Pending their appearance, we may regard the discussion of Müller's theory as brought to an end; and it does not appear doubtful with what conclusion: no theory can stand for a moment which has so many and so various and so powerful objections arrayable and arrayed against it. We ought not indeed, to leave out of sight the modestly hypothetical tone in which its author originally put it forward, adducing against it more than one consideration which, if he had taken the trouble carefully to weigh them, he would have seen to be fatal to it; and ending his whole exposition thus: "I shall be as much pleased to see my hypothesis refuted as to see it confirmed; all that I request for it is an impartial examination." But then he has, after all, written it down and put it forth, for the examination and criticism of scholars; and he must accordingly be held responsible for its character, and has no right to complain if it is treated just as its own intrinsic merits deserve. If he were also to suggest, as a hypothesis, for the discussion of comparative philologists, that a real analogy existed between Grimm's Law of rotation of the three classes of mutes and the nomenclature of these three trees; that the original names for 'fir,' 'oak,' and 'beech' had each been pushed one step around in the series; that, while *quercus* had changed from 'fir' to 'oak,' and *fāgus* from 'oak' to 'beech,' *abies* had also (though, owing to the loss of needed evidence, we could not so clearly prove it) certainly changed from 'beech' to 'fir'—then, with however many *ifs* and *provided's* he might season his exposition, whatever gratitude he might promise to the man who should convince him that his notion was a foolish one, its folly would remain incontestable, and he would deserve to be well laughed at for ever having

confessed to entertaining it seriously. Nor would the case be different if he were to put forward, with ever so many allowances that he was perhaps mistaken, the theory that the gradual contraction of the earth's crust had something to do with the universal abbreviation of the vocabular elements of speech: that the latter was, as it were, "the shadow cast on language" by that great event as it passed. Now Müller's modestly indicated hypothesis is just as unreasonable, just as inherently absurd, as either of those others would be; it differs from them only in that its absurdity does not lie so openly and palpably upon its very face. It is one of those queer imitations of a correspondence which now and then call a smile to a man's countenance when he chances upon them over his table; or with the recital of which he, at the utmost, amuses a friend in the course of an evening's walk. To devote to its exposition and support fifteen pages of a treatise on a new "Science," as Müller has done, is to make out of a joke a far too serious matter.

But Müller has done more than this. In a criticism of his volume, published eleven years ago,* I devoted two pages to a complete statement and refutation of the theory; bringing against it, in effect, nearly the same objections which have been here made, though otherwise cast, and in a much briefer form. I had given it, as I knew, a wholly "impartial examination;" and I conceived myself to be fairly earning an expression of the "pleasure" with which its author had promised to greet its refutation. Doubtless this was too sanguine; it might have been suspected that one who could frame and publish such a hypothesis would not be easily accessible to any brief and summary demolition of it; that he might probably enough even show toward it that instinctive special affection which mothers are said sometimes to feel for their weakest and least creditable offspring. At any rate, last year, in an article† which proves him not to have been in a state of mind to profit by any correction of mine, however honestly meant and faithfully administered, he makes what

* North American Review, vol. c., 1865; also Or. and Ling. Studies, i. 239-62.

† Chips, iv. 456 *et seq.*

he intends as a stout defense of his bantling. His answer to one of my three main points of objection I have already quoted in full: I had been ploughing with his heifer, and, urging the poor beast forward with unseemly haste. *En revanche*, he endeavors, in replying to another point, to impale me upon the horns of a heifer of my own. I have, he says, "unintentionally" offered him the "best illustration" of just such a change of meaning as he assumes in his tree-names, by showing* how the English settlers in America applied the old familiar names of *robin* and *blackbird* to new kinds of bird, somewhat resembling those which had borne them in England. But what is the analogy between this case and the one we have had in hand? Perhaps we have all along mistaken his meaning, and he has only intended to maintain that the Italic tribes migrated from a country where firs and oaks prevailed to one where, instead, only oaks and beeches were found, and therefore, having the 'fir' and 'oak' names idle on their hands, proceeded to apply the former to the oak and the latter to the beech, each being the species nearest like what the same word had before designated. This, and this only, would be a real parallelism. If we are to find one for the theory as originally put forward by Müller, we shall be obliged, it seems to me, to turn the American act of nomenclature the other way, and see the robin and blackbird "transferring the name of 'Indian,' as an appellative, to the new white people that were springing up in their wild homes."

It is needless to spend more time upon Müller's attempt to rehabilitate his hypothesis. About each item which he touches he raises a thin cloud of word-dust, just sufficient to obscure its outlines; showing an unclearness so curious and puzzling that one almost gives up in despair the endeavor to trace the mental state which it represents. Anything in the nature of a counter-argument no candid and competent person will pretend to discover. But he sings his little song of triumph at the end, and regards his fir-oak-beech theory as established more firmly than ever before the eyes of the students of language.

* Namely, in Or. and Ling. Studies, i. 303.

The matter to which we have thus given a few minutes' attention is, in one respect, an almost purely personal one. The scientific question involved is of quite insignificant importance; and no man of real knowledge and penetration is likely to be so far carried off his feet by Müller's persuasive eloquence as to take his hypothesis for an acceptable explanation. The fact that the whole thing is so curiously characteristic of Müller, and that Müller's personality is an element of high importance in the prevailing currents of thought and opinion on a host of subjects, is what gives the subject a wider and impersonal bearing. He has a real genius for exposition and illustration; this very note, "On words for fir, oak, and beech," is full of interesting facts, interestingly grouped, and may be read with lively pleasure by any one who can leave out of sight for what they are marshalled and to what end directed. What its author lacks is inductive logic, the power of combining his facts aright and seeing what result they yield; his collected material dominates and confuses him; often he hits the truth, with a kind of power of genial insight; often he hits wrong, sometimes perversely and absurdly wrong. No man needs to be studied with a more constant and skeptical criticism; no man is less worthy of the blind admiration and confidence, resembling that of a sect in its prophet, with which he has now long been regarded by an immense public, and even by scholars of a certain grade. While he maintains this false position, his influence is harmful, obstructive to the cause of truth; to do anything toward reducing his authority to its true value is a service to truth and to sound science.

There is yet another personal aspect which the controversy bears, personal to both Müller and myself; and I cannot forbear spending a few words upon it in closing. Since, for the reasons just laid down, I have repeatedly controverted views and arguments of his which appeared to me to be false—and sometimes sharply, as I thought they deserved—it was obviously for his interest, in lack of any other more effective method of reply and defense, to represent me as a mere fault-finder and personal vilifier. This he accordingly did, at considerable length, in the *Contemporary Review* for January,

1875. In the course of his article, he threw out against me this taunt: "He bitterly complains that those whom he reviles do not revile him again" (p. 314). In my answer, in the same Review (May, 1875; p. 728), I said: "If I stated that any one 'bitterly complained' that he was not answered by those he criticized, I should feel called upon to give chapter and verse for it; and neither Mr. Müller nor any one else can point out any such complaints on my part." It is in answer to the challenge here implied, to authenticate his charge, that Müller returns to the "words for fir, oak, and beech." The way of it is this.

In the preface to the sixth edition of his lectures (1871), Mr. Müller permitted himself a sneering reference to my criticism of an earlier edition, and a sophistical and untenable reply to one or two of the points which I had made against him. I therefore sent to the Review which had printed the original criticism (the *North American*) a rejoinder,* in which, after setting right the points referred to, and showing that the sneer was a gratuitous one, I went on thus: "We earnestly desire, and heartily invite, a continuation of his exposures. We should be glad, for example, to see him defend his explanation of the phenomena stated in Grimm's Law. . . . We should like, again, to have him try to prove that any one of the three impossible assumptions which we pointed out as involved in his argument respecting the 'names for fir, oak, and beech' does not vitiate that argument"—and so on. This challenge, now, this invitation to go on and set up again, if he could, the hypothesis which I claimed to have overthrown, is what he brings up, and the only thing that he brings up, to justify his former allegation that I had "bitterly complained" of not being reviled by those whom I had reviled! He does not, indeed, venture to repeat the allegation here in precisely the same terms (though he has reproduced it unaltered in his reprint of the *Contemporary* article in the "*Chips*," iv. 433); he speaks, rather, of "a passage where Professor Whitney expressed his dissatisfaction at not being answered, or, as I

* *North American Review*, vol. cxliii., 1871; reproduced in *Or. and Ling. Studies*, i. 262 *et seq.*

had ventured to express it, considering the general style of his criticism, when he is angry that those whom he abuses, do not abuse him in turn.* Then, in his summing up at the end, he asks, with all the conscious dignity of re-established blamelessness:† “Was I, then, so far wrong when I said that Professor Whitney cannot understand how anybody could leave what he is pleased to call his arguments, unheeded?” We may ask in return, was there ever seen a more beautiful instance of the *diminuendo* in accusation? The documentary and tangible “bitter complaint at not being reviled” first becomes a purely inferential “anger at not being abused”—incapable of absolute refutation, because the accuser, even when driven into the last corner, can still say: “Oh, I am quite sure that he was angry, though he did not show it;” then this is confessed to be a mere adventurous dysphemism for what, when strictly defined, is only a “dissatisfaction at not being answered;” and the last finally dwindles to an intellectual “failure to understand it that one’s arguments are unheeded.” Surely, the mutual interchanges of oaks and firs and beeches are nothing to this; it could only be paralleled by the transmutation of oaks into alders and of alders into bramble-bushes. In its last form, moreover, the statement, though less widely remote from truth, is not less strictly erroneous, than in its first; for I have not, in fact, ever been at a loss to understand why Müller left my arguments against his views so long unanswered; any more than why, when he finally attempted to answer them, he found nothing to use against them save evasion, misrepresentation, and detraction.‡

* Chips, iv. 500.

† Chips, iv. 504.

‡ The character of his charges in reference to points of Sanskrit grammar I have briefly exposed in the Proceedings of the American Oriental Society for May, 1876.

VI.—'Shall' and 'Should' in Protasis, and their Greek Equivalents.

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Professor Sewall, in his paper "On the Distinction between the Subjunctive and Optative Modes in Greek Conditional Sentences" in the 'Transactions' for 1874, and Professor Morris, in his paper "On Some Forms of Greek Conditional Sentences" in the 'Transactions' for 1875, have criticised especially that part of my classification of conditional sentences in which I maintain that the optative in ordinary protasis (i. e. in all conditions in which it does not express a past general supposition) is "merely a vaguer or less vivid form than the subjunctive for stating a future supposition." In opposition to this view, they agree in maintaining *some* form (though not quite agreed upon the precise form) of the distinction commonly made between the two moods in protasis, based upon the greater or less *possibility* or *probability* that is implied, or upon the presence or absence of an *expectation* or *anticipation* of the fulfilment of the condition. Professor Morris further suggests an important limitation to the use of the subjunctive in protasis, by expressing "a strong opinion that no case can be adduced from the best writers in which a future supposition demanding for its fulfilment *a violation of physical laws* is expressed by *ἐάν* with the subjunctive."

These attacks are both directed against what I have always felt to be the weakest point in my classification, a point on which I am myself a convert from the doctrines of my opponents. And although I find myself now unable to see the distinction which I once thought I could see, and which most scholars still think they can see, between *ἐάν γένηται* and *εἰ γένοιτο*, I am by no means disposed to be intolerant toward those who are of a different opinion. One gain has thus far come from the discussion—the clearer statement of one important point in the controversy; for I understand it to be generally admitted that the difference between *ἐάν* with the

subjunctive and *ei* with the optative is essentially the same as that between 'if he shall' and 'if he should' in English, and that if we can determine the principle that underlies the latter construction, we have the key to the former. The use which all scholars constantly make of these English forms to translate and explain the Greek constructions in question, whatever may be their theories of the latter, shows the general feeling on this point. If this is once admitted, it will aid us greatly in understanding the Greek form of protasis to ask ourselves what distinction we are in the habit of making between 'if he shall go' and 'if he should go' in English. But here unfortunately we meet an obstacle. The modern English, in which we think and express our thoughts, has substituted for the future form 'shall' in protasis the colorless present, so that we now say 'if he goes,' 'if he reads,' etc., for the more exact 'if he shall go' (or 'if he go'), 'if he shall read' (or 'if he read'), etc., which the translators of the Bible in the seventeenth century would have used. Further, this same present form, though we seldom use it to express a purely present condition (for which we should generally say 'if he is reading,' not 'if he reads'), is yet constantly used in general present conditions like 'if any one (ever) reads,' which are entirely distinct from the future conditions we are considering. We must therefore confine ourselves to cases in which we use either 'if he goes,' etc., in the sense of 'if he shall go,' etc., or the latter form itself. Fortunately the translation of the Bible makes every English scholar familiar with the older and more exact form, even if he never uses it in speaking or writing.

It will be understood that, when I compare the optative with the subjunctive in conditions in this paper, I shall confine myself to the optative in its fixed usage in Attic prose, excluding, for example, all notice of the present optative in Homer used to express a present unfulfilled condition, like the imperfect indicative in Attic Greek.

Professor Morris very properly asks for a more exact definition of the term 'vividness,' which I use in stating the distinction between the subjunctive and optative in protasis.

I have generally called the statement of a future condition which is made by the subjunctive, corresponding to 'if he shall go' (or 'if he go') in English, "more distinct and vivid" than that which is made by the optative, corresponding to 'if he should go.' By this I mean that the picture (so to speak) of the event or the circumstances supposed which is presented to the mind when the former expression is used is a "more distinct and vivid" one, a picture with outlines more sharply defined and more distinct and definite in its whole conception, than that which the latter form presents. On the other hand, as it seems to me, when the optative form is used, i. e. when we state a supposition in the form 'if he should,' the whole conception is vaguer and presents to the hearer a "less distinct and vivid" picture of the event supposed. For example, it seems to me that the supposition 'if some barbarian *shall* ever drag thee away weeping into slavery,' differs from 'if some barbarian *should* ever drag thee away weeping into slavery,' simply in this, that the former presents a more distinct and vivid conception of the event than the latter; and I do not believe that any one who had no theory of Greek syntax in view would ever think of distinguishing them by saying that one implies "an anticipation of the possible realization" of the supposition, while the other implies "an imagination of the possible realization" of the supposition; or that the one is "a supposition relating to contingent fact," while the other is "a supposition of conceived fact." I say merely that *it seems to me so*; and the more I think of the matter, the less I am able to see either of the last mentioned distinctions in the two expressions. If, however, others, when they use such expressions, feel that either of these distinctions is in their mind, it is impossible to appeal from this decision to any tribunal which will have higher authority with them. Let it be understood that I doubt whether any one who thought merely of the English expressions in question would ever make either of these distinctions between them *by his own suggestion*; I do not doubt that witnesses without number, if they were asked directly whether they *did not make* these distinctions, would

testify that they did so; for where it is so difficult to state or conceive clearly a distinction in language, it is extremely easy to imagine it to be almost any one which is plausibly given on high authority. We need not go far from the subject now under discussion for striking examples of this tendency. How many have quietly and in perfect confidence assented to the doctrine that the subjunctive in final clauses after past tenses "brings the action of its verb down to the present time," although there is hardly a page of Thucydides which would not demonstrate its utter absurdity! It seems to me, further, that the distinction of the optative as a "less distinct and vivid" form of expression than the subjunctive and equivalent forms (e. g. the imperative) appears in most of the constructions which admit the optative. In independent sentences, compare *μὴ πάθῃτε*, DEM. Lept. § 50 (p. 472), with the common *μὴ πάθοιτε*, the former being *do not suffer*, the latter *may you not suffer*. The same may be seen in the Homeric use of the independent optative compared with the imperative; e. g. in *Ἑλένην Μενέλαος ἄγοιτο*, Il. iv. 19, and *γυναικά τε οἰκαδ' ἀγέσθω*, Il. iii. 72, the former being *may he carry*, the latter *let him carry*. In the double construction of oratio obliqua after past tenses, where an option is allowed between a subjunctive of the direct form and the same tense of the optative, the latter is evidently the weaker and less vivid form of expression, differing in no other respect from the former. The fact that two cases of this distinction have been inadvertently cited to illustrate the assumed distinction between the subjunctive and optative in protasis, although the direct forms in both cases would have been identical in construction, has been already used by me* to confirm my argument against admitting any other distinction in direct discourse than is generally allowed to exist in these indirect

* See the remarks in *Transactions* for 1873, p. 72, on *εἰ συμπεῖθαι* and *εἰ εἰσῃγοῖτο* as compared with *ἐὰν αἰρηθῇ* and *ἐὰν ᾗ* in DEM. Cor. p. 276, §§ 147, 148, where the two optatives are due entirely to the oratio obliqua and represent *ἐὰν συμπεῖθῃ* and *ἐὰν εἰσῃῇ* of the direct discourse. And yet these are standing examples of the "essential and inherent distinction" between the subjunctive and the optative! See, for instance, Disson's and Holmes's notes on the passage, and Kühner, § 576, Anm. 7

quotations. The principles of oratio obliqua, as regards the choice of moods, apply, as I have already shown,* to the distinction between the subjunctive and optative in final and object clauses (with *ἵνα*, *ὥπως*, *μή*, etc.) after past tenses, to which I have referred above. These analogies drawn from the other uses of the optative have given a strong and (I may add) an unexpected confirmation to the opinion to which I was led originally by a consideration of the subjunctive and optative in protasis and relative clauses alone.

If now the distinction which I have tried to establish is the true one, the question recurs, when will a speaker naturally use the subjunctive and when the optative in stating a future condition? In most cases he will use the more vivid form to express a supposition which *for any reason* is more vividly conceived and so more prominent in his own mind, or one which he wishes to bring more distinctly before the mind of the hearer; and the less vivid form for one which for any reason is less prominent or which he wishes to present less distinctly. His choice, therefore, may be influenced by various considerations. He will naturally form a more vivid conception of a supposition which he thinks highly probable in its nature or likely to be realized in a particular case, or of one which he especially desires or especially dreads to have realized. He will naturally express with greater distinctness a supposition which he wishes to mark as especially absurd; or one which for any reason he wishes to make especially emphatic in comparison with others in the same sentence, whatever may be the nature of the supposition itself; while he will naturally express with less distinctness one which he wishes to make less emphatic. Cases in which the subjunctive and optative in protasis are brought into contrast in successive sentences are very rare, so that we can generally supply the alternative form only in imagination. It must be remembered too that neither the subjunctive nor the optative expresses any *absolute* amount of vividness or distinctness, still less any absolute amount of probability or desire; these qualities are merely relative, and are made

* See *Transactions* for 1873, p. 73.

obvious chiefly by contrast. We must not be surprised, therefore, to find precisely the same supposition expressed in different forms by different persons who need not differ in their opinion of the nature of the supposition, or by the same person at different times without any necessity of a change of mind on his part: of this examples will be given below. Now if the distinction between the two moods were essential and fundamental, as Professor Sewall and Professor Morris believe it to be, it seems to me hardly possible that this variety of expression could be allowed: in that case, most conditions would fall by a fixed principle into one class or the other, and any change in the form would involve a grammatical error of the same nature (though of course not of the same degree) as that which an Athenian would have committed if he had said *εἰ ἔλθοιμι* in the sense of 'if I had gone.' There are some conditions, involving an extreme amount of absurdity or improbability, which would more naturally be stated by the optative alone unless special emphasis were intended; to this class belong most of the excellent and pertinent examples collected by Professor Morris,—*if the house should find a voice,—if the moon should never rise again,—if I should go on with my story for ten days,—if they should get a power like that of Gyges,—if a man should have three talents of gold in his stomach, one in his head, and a stater of gold in each eye.* In English as well as in Greek such conditions would in most cases be stated in the vaguest possible form, to correspond to the vagueness of such conceptions in the mind. But I hope to show below that all these conditions might under certain circumstances be stated in the more vivid form, without involving any grammatical or logical absurdity.

Before proceeding to state another consideration which often influenced the choice of mood in conditions, I will give examples of suppositions in which the choice of mood appears to be affected by one or more of the considerations already mentioned.

1. In Plato's Republic (vi. 494 B—E) we have a famous description of the career of a bright and handsome young

man, of high birth and great wealth, exposed to the flattery and adulation of a populous city, and of the fate of any philosophic friend who may attempt to divert him into the path of wisdom. We should say that this might be selected as a strong case of "conceived fact" as opposed to "contingent fact," or of the "imagination" rather than the "anticipation" or "expectation" of the condition being realized. If this case had been supposed in the optative form, all would have called it a striking instance of a purely ideal supposition. But here it is plain that Plato had in mind the career of Alcibiades and the relation of the fast young Athenian to Socrates; and he adds a most striking dramatic effect to his sketch by making Socrates imagine the course of the young man in the more graphic and impressive form of supposition. I feel sure that Professor Morris will agree with me in saying that it was optional with Plato to give or withhold this artistic touch; and we shall agree in thinking that the use of the subjunctive makes the sketch more life-like and implies that it is less of a fancy sketch than the optative would have done. He will, however, maintain (I fear) that the "expectant" form, the subjunctive, implies *by its own nature* necessarily a looking forward to realization; while I hold that the "vivid and distinct" form can be used to express emphasis in many other ways, even when there is no thought of realization; as in I. Cor. xii. 15, 16: *ἐὰν εἴπῃ ὁ πούς, if the foot shall say; ἐὰν εἴπῃ τὸ οὖς, if the ear shall say*; or in Matth. xv. 14: *τυφλὸς δὲ τυφλὸν ἐὰν ὁδηγῇ, ἀμφότεροι εἰς βόθυνον πεσοῦνται, and if the blind lead the blind, both shall fall into the ditch*. Let us see what effect would be produced by a change of mood: e. g. by substituting *εἰ τις ἡρέμα προσελθὼν τάληθ' ἔλεγ'οι, . . . ἂρ' εὐπετὲς ἂν οἶε εἶναι εἰσακοῦσαι*; for *ἐὰν τις λέγῃ, κ. τ. λ., i. e. if some one should go to him and tell him the truth, for if some one shall go to him and tell him the truth*. I can see in the former only the natural form of expression for such a supposition, which any of us would use in a similar case, and which any Greek would have used who had never known a career like the one supposed or who had no desire to make his sketch particularly impressive; in the present case, however, Plato wishes to

paint as impressive a picture as he can of a most striking historical event, and he therefore uses a more vivid form of statement, precisely as an artist might have used brighter colors in a real picture for an analogous purpose. That Plato could not have felt that the optative form would have been absurd here appears plainly from a parallel passage (Rep. vii. 517 A), where Socrates is made to refer to himself quite as pointedly as before, under the character of the man who attempts to release the prisoners in the cave and to lead them up to the light, and whom they would kill *if they could in any way get him into their hands*, εἰ πως ἐν ταῖς χερσὶ δύναιντο λαβεῖν καὶ ἀποκτείνειν, ἀποκτείνειν ἄν. This could have been expressed by ἐάν πως δύνωται ἀποκτενεῖν, i. e. *will they not kill him if they can?* If it had this form, it would be simply a more lively picture of the fate of Socrates than we now have, and this would explain (as in the other case) what might otherwise seem too distinct and vivid a statement of a condition which in itself seems eminently fitted for the other form of expression.

2. In PLAT. Gorg. 521, 522, Socrates is represented as predicting his own trial and condemnation; and in contrast with this definite foreboding he supposes, merely for illustration, the case of a physician tried by a jury of boys with a pastry-cook as accuser. The outline of the construction is as follows (521 C—522 A): τὸδε μέντοι εὖ οἶδ' ὅτι, ἐάνπερ εἰσὶν εἰς δικάστηριον, πονηρὸς τίς με ἔσται ὁ εἰσάγων· καὶ οὐδέν γε ἄτοπον (sc. ἂν εἴη) εἰ ἀποθάνοιμι. . . . οὐχ ἔξω ὃ τι λέγω ἐν τῷ δικαστηρίῳ. κρινοῦμαι γὰρ ὡς ἐν παιδίοις ἰατρὸς ἂν κρίνοιτο κατηγοροῦντος ὀψοποιοῦ. σκόπει γὰρ, τί ἂν ἀπολογοῖτο ὁ τοιοῦτος ἄνθρωπος ἐν τούτοις ληθθεῖς, εἰ αὐτοῦ κατηγοροῖ τις λέγων ὅτι, κ. τ. λ. . . . τί ἂν οἶε ἔχειν εἰπεῖν; ἢ εἰ εἴποι τὴν ἀλήθειαν, ὅποσον οἶε ἂν ἀναβοῆσαι τοὺς τοιούτους δικαστάς; Here we have a marked distinction between the more vivid form in which Socrates imagines himself brought before a court and the less vivid conception of the physician on his trial; and I believe that this distinction was based upon anticipation in one case and imagination in the other, a contrast in feeling which found its most natural expression in this contrast of forms. Further, I have no doubt that the change to εἰ ἀποθάνοιμι in the second supposition, where ἐάν ἀποθάνω would certainly

have been permitted, if not expected, indicates a less vivid anticipation of being condemned to death than of being brought to trial. Just below (522 B) he applies the comparison to his own case by an apodosis in the optative: *τοσοῦτον μέντοι καὶ ἐγὼ οἶδα ὅτι πάθος πάθοιμι ἂν εἰσελθὼν εἰς δικαστήριον*. But he returns immediately to the other form, with which he began: *οὔτε γὰρ ἡδονὰς . . . ἔξω λέγειν . . . ἂν τέ τις με ἢ γεωτέρους φῇ διαφθεῖρειν . . . ἢ τοὺς πρεσβυτέρους κακηγορεῖν . . . οὔτε τὸ ἀληθὲς ἔξω εἰπεῖν . . . οὔτε ἄλλο οὐδέν· ὥστε ἴσως ὅ τι ἂν τύχῃ τοῦτο πείσσομαι*. In replying to the next question of Callicles, however, Socrates uses the optative form in reference to the same subject, his anticipated trial: *εἰ μὲν οὖν ἐμέ τις ἐξελέγχοι ταύτην τὴν βοήθειαν ἀδύνατον ὄντα ἐμανθῶ καὶ ἄλλῃ βοηθεῖν, αἰσχυνοίμην ἂν . . . , καὶ εἰ διὰ ταύτην τὴν ἀδυναμίαν ἀποθνήσκωμι, ἀγανακτοίην ἂν· εἰ δὲ κολακικῆς ῥητορικῆς ἐνδεία τελευτῶν ἐγωγε, εὖ οἶδα ὅτι ῥαδίως ἴδοις ἂν με φέροντα τὸν θάνατον*. All these conditions could have been expressed in the subjunctive form quite as properly as the earlier ones; indeed the last one, *εἰ δὲ . . . τελευτῶν* might naturally have had a subjunctive to express contrast with the preceding *εἰ μὲν*, κ. τ. λ., since one makes a supposition abhorrent to the speaker's feelings, which he regards as impossible, while the other refers to what actually took place and had already taken place when Plato wrote the words. It seems to me that no theory of the two forms of condition which assumes that there is in almost all cases a predetermined form in which alone a given future condition can be properly expressed can be applied consistently to these cases.

3. In PLAT. Phædr. 259 A, Socrates imagines that the cicadae are watching his conversation with Phaedrus to see whether their chirping will lull the speakers into a noonday nap. He naturally hopes this will not be the case; and his change from the less vivid to the more vivid form of supposition seems to indicate this hope. He says: *εἰ οὖν ἴδοιεν καὶ νῦ καθάπερ τοὺς πολλοὺς ἐν μεσημβρίᾳ μὴ διαλεγόμενους, ἀλλὰ νυστάζοντας καὶ κηλουμένους ὑφ' αὐτῶν δι' ἀργίαν τῆς διανοίας, δικαίως ἂν καταγελῶν . . . ἂν δὲ ὁρῶσι διαλεγόμενους . . . τάχ' ἂν δοῖεν ἀγασθέντες*. It may perhaps be thought that the antithesis here implies a stronger expectation of the latter condition being realized; and such examples are too rare to decide the question. In

most cases, however, in which expectation, desire, or hope is more prominent in one of the two successive conditions, the same form is found in both, as in DEM. Cor. § 178 (pp. 287, 288): *ἐὰν μὲν δέξωνται ταῦτα καὶ πεισθῶσιν ἡμῖν*, and *ἂν δ' ἄρα μὴ συμβῇ κατατυχεῖν*.

4. Cases of the more vivid form in suppositions the realization of which the speaker strongly dreads, and is trying by his argument to prevent, are found in DEM. Aphob. I. § 67 (p. 834): *ἐὰν γὰρ ἀποφύγῃ με οὗτος, ὃ μὴ γένοιτο, τὴν ἐπωβελίαν ὀφλήσω μνάς ἑκατον*. But the use of this form was optional here also; for we find in Aph. II. § 18 (p. 841) *ποῖ δ' ἂν τραποίμεθα, εἴ τι ἄλλο ψηφίσαισθε;* (referring to the same danger of an adverse vote), and within three lines of this we have *τούτου γίγνεται, τὴν ἐπωβελίαν ἐὰν ὀφλωμεν*, and still again in § 21 (p. 842), referring to the orator's sister in the same contingency, *εἰ δ' ὑμεῖς ἄλλο τι γνώσεσθε, ὃ μὴ γένοιτο, τίνα οἴεσθε αὐτὴν ψυχὴν ἔξειν, ὅταν ἐμὲ μὲν ἴδῃ*, κ. τ. λ.; I shall speak of these passages again below.

5. Cases in which the more vivid form is chosen to heighten the absurdity of an already absurd supposition sometimes occur, although they are naturally rare. Such seems to me to be PLAT. Repub. X. 610 A: *ἐὰν μὴ σώματος πονηρία ψυχῇ ψυχῆς πονηρίαν ἐμποιῇ*, *unless a bodily vice shall engender in a soul a mental vice*,—a supposition which is at once stigmatized as absurd in 610 C: *τοῦτό γε οὐδεὶς ποτε δεῖξει*. Even after this the supposition follows: *ἐὰν δέ τις ὁμόσῃ τῷ λόγῳ τολμᾷ ἰέναι καὶ λέγειν*, κ. τ. λ. Again, in 612 B, it is said of the soul *καὶ ποιητέον εἶναι αὐτῇ τὰ δίκαια, ἐάν τ' ἔχῃ τὸν Γύγου δακτύλιον ἐάν τε μὴ*, καὶ πρὸς τοιούτῳ δακτυλίῳ τὴν "Αἴδος κυνὴν, i. e., *that the soul must do what is just, whether she have or have not the ring of Gyges, and besides such a ring the cap of Hades*. In a previous passage (359 c) the former miracle had been mentioned in the other form: *εἰ αὐτοῖς γένοιτο οἷαν ποτέ φασι δύναμιν τῷ τοῦ Ἀνδρῶ προγόνῳ γενέσθαι*, and again (360 B): *εἰ οὖν δύο τοιούτῳ δακτυλίῳ γενεοίσθην*, κ. τ. λ., but with less emphasis. See also EURIP. Phoeniss. 1215, 1216: ΑἴΓ. *οὐκ ἂν γε λέξαιμι' ἐπ' ἀγαθοῖσι σοῖς κακά*. ΙΟΚ. *ἢν μὴ γε φεύγων ἐκ φύγῃς πρὸς αἰθέρα*. And Orest. 1592, 1593: ΟΡ. *φησὶν σιωπῶν ἄρκέσω δ' ἐγὼ λέγων*. ΜΕΝ. *ἀλλ' οὐτὶ χαίρων, ἢν γε μὴ φύγῃς πτεροῖς*. Under this head, as it seems to me,

might very properly come a Greek version of the proverb, 'If the sky fall, we shall catch larks,' in which the absurdity of the condition is heightened by expressing it in the more vivid form in English, and I feel confident (after carefully considering Mr. Morris's argument) that the effect would be the same in Greek. Similar to the examples just quoted is the sarcastic reply of Socrates (PLAT. Gorg. 470 c) to the taunt of Polus that even a child could show him to be in the wrong; to which Socrates replies: πολλὴν ἄρα ἐγὼ τῷ παιδί χάριν ἔξω, ἴσην δὲ καὶ σοί, ἐάν με ἐλέγξης καὶ ἀπαλλάξης φλυαρίας, i. e. *I shall be much obliged to the child, and equally so to you too, if you shall refute me*, etc. I will refer also to two conditions from PLAT. Euthyd. 299 B, C, which are quoted below (p. 101), one of which supposes a cartload of hellebore to be given at one dose, and the other supposes the patient who drinks it to be as big as the "statue at Delphi." Both are expressed by the subjunctive with ἐάν.

6. Occasionally the subjunctive form seems to mark a supposition as more emphatic than others with which it is contrasted, and the optative form to mark one as less emphatic than others, when there is no apparent distinction on the score of probability, expectation, desire, fear, or sarcasm. Thus in PLAT. Protag. 330 c—331 A we have a series of conditions stated by Socrates in the optative form: εἰ τις ἔροιτο ἐμέ, ἀποκρίναίμην ἄν,—εἰ οὖν μετὰ τοῦτο ἔροιτο, φαίμεν ἄν,—εἰ οὖν εἴποι, εἴποιμ' ἄν,—εἰ οὖν εἴποι, τί ἄν ἀποκρίναιο; But here all at once he changes to the subjunctive form, and says: τί οὖν ἀποκρινούμεθα αὐτῷ, ταῦτα ὁμολογήσαντες, ἐὰν ἡμᾶς ἐπανεῖρηται, κ. τ. λ.; The argument had here reached a point at which Socrates felt he had gained an advantage, and he therefore puts this question with special emphasis. The whole conversation is purely imaginary, and certainly there was no greater probability or expectation of this question being asked than of the others, which indeed were a necessary introduction to this; the simple truth, as it seems to me, is that a more vivid form was chosen to state a supposition which was to be made more prominent in the argument than the others. After this vivid statement of the condition, with a repetition of the apodosis τί αὐτῷ ἀποκρινούμεθα; Socrates

returns to the other form and says: *ὑπὲρ γε ἑμαντοῦ φαίην ἄν...* καὶ ὑπὲρ σοῦ δὲ, εἴ με ἐφής, ταῦτ' ἂν ταῦτα ἀποκρinoίμην. In PLAT. Crit. 51 D the laws are supposed to say: καὶ οὐδεὶς ἡμῶν... ἀπαγορεύει, ἂν τέ τις βούληται ἡμῶν εἰς ἀποικίαν ἵέναι, εἰ μὴ ἄρ' ἐσκοιμεν ἡμεῖς τε καὶ ἡ πόλις, ἂν τε μετοικεῖν ἄλλοσέ ποι ἔλθων, ἵέναι ἐκεῖσε ὅποι ἂν βούληται, ἔχοντα τὰ αὐτοῦ. There the single optative seems to indicate a condition which is less emphatic than the main one, although there is no other apparent ground for the change of form. We may produce the same effect in English: *and if any one of you shall want to go off to some colony,—supposing we and the state should fail to please him,—or if he shall want to go to some foreign country and live, none of us forbid him to go, etc.* No one would have been offended surely if any of the conditions quoted under this head had been expressed in the other form.

In the passages already quoted, the choice of mood in the protasis appears to have been affected more or less by the considerations mentioned, sometimes perhaps by several of them at once, and in each case some peculiar effect is produced by the mood chosen. Now it seems to me that these various considerations can hardly be reduced to the single one of 'probability,' 'expectation,' or 'anticipation of realization,' although I admit that this is one of the most common grounds of distinction where any can be seen. I have already stated (*Transactions* for 1873, p. 71) that this and the other grounds of distinction "seem to me to stand to the more comprehensive one of greater and less vividness in the relation (if I may be allowed the expression) of species to a genus"; in other words, I think the mistake commonly made here lies in confounding a very common (perhaps the *most* common) use of the distinction between the subjunctive and optative in protasis with the distinction itself. It will hardly be denied, I think, that, with the exception of the one relating to the physician before the jury of children, in PLAT. Gorg. 521 E (of which below), all these conditions could have been stated in the other form without *essential* change of meaning, though often not without the loss of some special emphasis or effect. Now, if there were "a distinction in essence and fundamental"

between the two forms, I hold that this interchange would be impossible, except on the assumption that the examples quoted are exceptional and too infrequent to cast doubt on an established principle of the language. But if the distinction is such as I have stated it, this interchange is just what would naturally be expected.

It will still be urged, however, that exceptions are as fatal to my principle as to the other, and that, if there is no essential and fundamental distinction between the two forms, every future supposition should admit of a double statement. I have indeed said that most of the conditions quoted by Professor Morris are more naturally stated in the optative form, because this vaguer form is in most cases better adapted to an improbable supposition, which must needs be more vaguely conceived than one which is distinctly anticipated. But I have given cases of the subjunctive in conditions which are quite as improbable and even absurd as any in Mr. Morris's list. Surely 'escaping into the air,' 'taking flight on wings,' and 'having the ring of Gyges with the cap of Hades' could not be expressed by the subjunctive if absurdity or violation of physical laws were a bar. And yet every one must feel, with Professor Morris, that most of his examples could not be changed to the subjunctive form without violence to the thought. Why now is this so? Even if it is said that the subjunctives just mentioned are due to "rhetorical effect" (which has long been a *deus ex machina* in Greek syntax), the question remains, why will not this potent agency transform Mr. Morris's examples for me as well as my own? To begin with the strongest case, AESCH. Agam. 37: οἶκος δ' αὐτὸς, εἰ φθογγὴν λάβοι, σαφέστατ' ἂν λέξειεν, and the parallel passage in PLAT. Protag. 361 A, where it is said of the issue of the argument: εἰ φωνὴν λάβοι, εἰπεῖν ἂν, κ. τ. λ.,—I would remark that there is nothing intrinsically more impossible in a house or the issue of an argument speaking than in laws speaking; and yet in PLAT. Crit. 50 c we find τί οὖν, ἂν εἰπωσιν οἱ νόμοι; The supposition of the laws addressing Socrates had first been made by εἰ ἔροιντο (50 A). Now why is there no absurdity in this sudden change to the more vivid

form? Merely because the apodosis is a simple future $\tau\acute{\iota}$ (sc. $\epsilon\pi\omicron\upsilon\mu\epsilon\nu$); so that the whole sentence means *what shall we reply if the laws (shall) say?* If, on the contrary, the sentence were *they* (the laws) *would astonish us by their eloquence if they should speak*, there would be the same objection to changing this to the subjunctive form which is felt in the other case of the house speaking. To say *the laws will astonish us by their eloquence if they shall speak to us* would be felt at once to be unnatural; but there is, as we have seen, no valid objection to be made to the protasis. So also in the changed form of the passage from Aeschylus the apodosis *the house will speak most plainly* is the only objectionable part; and this offends us because 'will speak' is too absolute and unqualified an assertion to make of a house, the more contingent and weaker form 'would speak' being the only one appropriate under ordinary circumstances. If now we substitute an apodosis here in which a simple future can stand, e. g. *dark deeds will come to light*, then grammatically (though not dramatically) all objection to $\eta\nu\ \phi\theta\omicron\gamma\gamma\eta\nu\ \lambda\acute{\alpha}\beta\eta\ \omicron\iota\kappa\omicron\varsigma$ is felt to be removed. Compare "Foul deeds *will* rise, though all the earth o'erwhelm them."

In ARISTOPH. Nub. 754: $\epsilon\acute{\iota}\ \mu\eta\kappa\acute{\epsilon}\rho'\ \alpha\nu\alpha\tau\acute{\epsilon}\lambda\lambda\omicron\iota\ \sigma\epsilon\lambda\acute{\eta}\nu\eta\ \mu\eta\delta\alpha\mu\omicron\upsilon\,,\ \omicron\upsilon\kappa\ \alpha\nu\ \alpha\pi\omicron\delta\omicron\iota\eta\nu\ \tau\omicron\upsilon\varsigma\ \tau\omicron\kappa\omicron\upsilon\varsigma$, the optative is perhaps necessary, as Mr. Morris intimates, to account for the patience of Socrates with the stupidity of his pupil, which would (he thinks) have been intolerable in the subjunctive form; nevertheless, whatever grammatical or logical objection there may be to the subjunctive will be at once removed if we substitute in the apodosis $\omicron\upsilon\ \mu\epsilon\ \delta\epsilon\acute{\iota}\eta\sigma\epsilon\iota\ \alpha\pi\omicron\delta\omicron\upsilon\nu\alpha\iota\ \tau\omicron\upsilon\varsigma\ \tau\omicron\kappa\omicron\upsilon\varsigma$ (I will not be tempted by Mr. Morris's success in verses to rush madly in *celeris iambos*). If a similar change is made in all the examples in the next fifty lines, to which Mr. Morris refers, no one can object to stating the conditions in the subjunctive except on the excellent ground that their impudence would be thereby greatly enhanced.

In AESCH. Pers. 431 the same principle holds; and if the apodosis be made future, e. g. *half my tale will not be told*, the subjunctive can stand in the protasis. I may add that, in

my own judgment, the changed form which Professor Morris gives as erroneous is not only correct, but elegant.

Of the next example (PLAT. *Repub.* II. 359 c) I have already spoken; and I think it will be evident that this could have been stated ἔσται δ' ἐξουσία... ἣν αὐτοῖς γένηται.

As to the striking example from PLAT. *Euthyd.* 299 ε, already mentioned: εἴη ἂν εὐδαιμονέστατος, εἰ ἔχοι χρυσοῦν μὲν τρία τάλαντα ἐν τῇ γαστρὶ, τάλαντον ἐν τῷ κρανίῳ, στατήρα δὲ χρυσοῦ ἐν ἑκατέρῳ πώφθαλμῳ;—I am encouraged to think that I am right in saying that it might be written ἔσται... ἐὰν ἔχη; by two conditions which precede (299 β and γ): καὶ καλῶς ἐκεῖ ἔξει, ἐὰν τις αὐτῷ τρίψας ἐγκεράσῃ ἑλληβόρου ἄμαξαν; to which Ctesippus adds: πάνυ γε σφόδρα, ἐὰν ᾗ γε ὁ πίνων ὅσος ὁ ἀνδρίας ὁ ἐν Δελφοῖς. Surely whoever can swallow this "cartload of hellebore" will not be troubled even by "three talents of gold in his stomach!" I may add here, as a proof that no amount of absurdity or impossibility can make the subjunctive incorrect in protasis, DEM. *Phil.* III. § 68 (p. 128): ὥπτε, μὴδ' ἂν ὅτι οὖν ᾗ, δεινὸν πείσεσθαι, where ὅτι οὖν is a sort of *x* for which we are at liberty to substitute anything imaginable. The more common formula would undoubtedly be οὐδ' ἂν εἰ ὅτι οὖν γένοιτο, but here the irregular future infinitive after ὥπτε makes the subjunctive in the dependent clause more natural.

In the argument cited from PLAT. *Phaed.* 72 β, γ, we cannot, it is true, suppose the conditions to be changed to the subjunctive form without injury to the argument, because the apodoses are not of a kind to be stated absolutely in the future indicative; but I can see nothing in the conditions themselves which would be repugnant to the other form. In *Phaedr.* 245 δ, however, I cannot doubt that either form could be used in the sentence εἰ γὰρ ἔκ του ἀρχῇ γίγνοιτο, οὐκ ἂν ἐξ ἀρχῆς γίγνοιτο. For in the corresponding sentence just below, which on every ground should be parallel in construction, we have ἀρχῆς γὰρ ὃ ἄπολόμενης, οὔτε αὐτὴ ποτε ἔκ του οὔτε ἄλλο ἐξ ἐκείνης γενήσεται, where the participle is clearly equivalent to ἐὰν ἀπόλῃται.

In the quotation from Pericles in ARIST. *Rhet.* III. 10, 7 (where three of Bekker's four Mss. read ἐξέλη), the optative seems clearly the more natural form, not only from the nature

of the supposition, but from the implied apodosis, which would be in the optative with ἄν. With an appropriate apodosis, I think even this condition could have the other form.

In XEN. Anab. III. 2. 24, the last of Mr. Morris's examples, I cannot see anything in the protasis, καὶ εἰ σὺν τεθρίπποις βούλονται ἀπιέναι, to exclude the subjunctive form; but the apodoses are all better expressed by the optative with ἄν than they would be by the future indicative. Indeed, it may be safely said that the implied protasis which conditions the first two optatives is also understood with the third, so that the expressed protasis states only part of the condition.

I fear that the doctrine of the effect of the apodosis upon the protasis may be considered even more heretical than the main proposition which I am defending. I must therefore give a few examples to illustrate this effect. It is especially evident in conditional relative sentences when the apodosis precedes the protasis and consists of an optative in a wish: in such cases the force of the optative in assimilating the dependent verb will be generally admitted. As examples may serve Odyss. I. 47: ὡς ἀπόλοιτο καὶ ἄλλος ὅ τις τοιαῦτά γε ῥέξοι, *may any other man likewise perish who shall do the like of this*; and MIMNERMUS Fr. 1: τεθναῖην ὅτε μοι μηκέτι ταῦτα μέλοι, *may I die when I shall no longer care for these*. Here few will deny that if the wish had been expressed by any other form than the optative—even by a weaker expression, like βούλομαι with an infinitive—the dependent verbs would have naturally been in the subjunctive, without any essential change in meaning. In Il. v. 212–15 we have one protasis in the subjunctive preceding the apodosis (an optative in a wish), and another in the optative following it: the assimilation is here very marked.

εἰ δέ κε νοστήσω καὶ ἐσόψομαι ὀφθαλμοῖσιν
πατρίδ' ἐμὴν ἄλοχόν τε καὶ ὑπερεφές μέγα δῶμα,
αὐτίκ' ἔπειτ' ἀπ' ἐμέϊο κάρη τ' ἄ μοι ἀλλότριος φῶς,
εἰ μὴ ἐγὼ τάδε τόξα φαιινῶ ἔν πυρὶ θεῖην.

But in Il. II. 258–261, a passage otherwise parallel, the assimilation is not effected. In AESCH. Prom. 979: εἴης φορητὸς οὐκ ἄν, εἰ πράσσοις καλῶς, *you would not be endurable if you*

should ever be in prosperity, who can doubt that a change of εἷς ἂν to ἔσται would have caused a change of εἰ πράσσεις to ἦν πράσσης, *if you shall ever be in prosperity?* In DEM. Aph. II. § 18 (p. 841), quoted above (p. 96): ποῖ δ' ἂν τραποίμεθα, εἴ τι ἄλλο ψηφίσαισθε; what possible reason can be given for the optative in a condition which is twice expressed by the subjunctive and once by the future indicative, except the assimilating force of the apodosis? The same effect is quite as striking in English as in Greek. We should say 'Turkey will beg for mercy, if Russia shall take (takes) Constantinople'; but we should also say, with no change in our view of the contingency, 'England would be in danger of war, if Russia should take Constantinople.' The form which the apodosis takes (which may be determined by various considerations not affecting our view of the realization of the condition which is to follow) in such cases naturally determines the form of the dependent protasis. That is, the greater or less absoluteness with which we state the apodosis often (though not always) affects the "distinctness and vividness" with which we state the same condition at different times.

Though this assimilating effect is more apparent and probably more powerful upon a protasis which follows its apodosis, it is by no means confined to such cases. I cannot see any other ground than assimilation for the distinction in the two conditions in Odyss. VIII. 352, 353, and 355, 356:—

πῶς ἂν ἐγὼ σε δέοιμι μετ' ἀθανάτοισι θεοῖσιν,
εἰ κεν Ἄρης οἴχοιτο χρέος καὶ δεσμὸν ἀλύξας;

"Ἡφαιστ', εἰ περ γάρ κεν Ἄρης χορείως ὑπαλύξας
οἴχηται φεύγων, αὐτός τοι ἐγὼ τάδε τίσω.

Here Poseidon, the last speaker, who proposes to be surety for Ares, would naturally be expected to state his supposition in the weakest form; but the apodosis τίσω, *I will pay*, is an absolute statement compared with δέοιμι ἂν, and this decides the form of the protasis by an influence as strong and as unfelt (by the speaker) as that which would cause every schoolboy to translate one protasis by *if Ares shall depart* (or *departs*), and the other by *if Ares should depart*, without

dreaming of one expressing more probability or implying more expectation than the other. I am sorry that I cannot see the ground for the distinction between the two suppositions of Philip's death, ἂν οὐτός τι πάθῃ and εἰ τι πάθῃ, in DEM. Phil. i. §§ 12, 13 (p. 43), for which Professor Morris argues so persuasively; but I cannot be convinced that an orator could use *within a single minute* and with reference to the same future contingency two forms of expression which differed essentially and fundamentally in the manner in which the supposed event was conceived, with regard to its probability or to the expectation or anticipation of its realization. I believe, on the contrary, that here too the protasis was assimilated in each case to the apodosis: in the former case this was ταχέως ἔτερον Φίλιππον ποιήσετε; in the latter it was a complicated sentence consisting of two distinct apodoses in the optative with ἂν, each conditioned specially by a participle. If the former apodosis had been ποιήσαιτ' ἂν (as it might easily have been), and the latter had been simply οὐδὲ Ἀμφίπολιν δέξασθαι δυνήσεσθε, I feel confident we should have had the two forms of protasis reversed.

The strong evidence I have given of the effect of the apodosis on the form of the protasis will, I trust, strengthen the position already taken (p. 90), that the subjunctive differs from the optative in common future conditions very much as it does in oratio obliqua after past tenses in conditions which in direct discourse have the subjunctive, and in the indirect form allow either subjunctive or optative. This is also an effect of the leading verb on the dependent mood, which is as plain in English as in Greek. Thus we say 'he says he will tell her *if she comes*;' but 'he said he would tell her *if she came*' (i. e. '*should come*'); corresponding to the Greek εἰν ἔλθῃ and εἰ ἔλθοι. The distinction of the Greek is that its greater freedom allows both εἰ ἔλθοι and εἰν ἔλθῃ in the latter case, while the English allows only the weaker form; that is, in Greek the assimilation is optional, in English it is compulsory. Perhaps the most striking cases of this principle in Greek are those in which the apodosis and the leading sentence on which the oratio obliqua depends are united in

one sentence; as in DEM. Cor. § 145 (p. 275): οὐκ ἦν τοῦ πρὸς ἡμᾶς πολέμου πέρας οὐδ' ἀπαλλαγὴ Φιλίππῳ, εἰ μὴ Θηβαίους καὶ Θετταλοὺς ἐχθροὺς ποιήσειε τῇ πόλει, i. e. *Philip saw no way of ending or escaping the war, unless he should make, etc.* Here ἰάν μὴ ποιήσῃ would have been equally proper, as is plain from sentences like THUC. II. 24: ἦν δέ τις εἶπερ ἢ ἐπιψηφίσῃ κινεῖν τὰ χρήματα... θάνατον ζημίαν ἐπέθεντο. I am very skeptical of any theory which assumes a more fundamental distinction between ἦν ποιήσῃ and εἰ ποιήσειε in ordinary cases than is universally admitted to exist here.

The same principle is illustrated by the double form of protasis allowed after final clauses which depend on past tenses; but this is really another form of protasis in oratio obliqua. See DEM. Aph. I. § 53 (p. 830); THUC. I. 58, 91.

I must leave many points of detail both in Professor Sewall's and in Professor Morris's paper unnoticed. I will briefly allude in conclusion to what seems to me a fatal objection to the system of classification advocated in both those papers. The fundamental idea assigned to the subjunctive, that of "contingency" or that of "anticipation" or "expectation" of realization, in my opinion, fails utterly to explain the nature of the "present general suppositions" expressed by ἰάν and the subjunctive. Professor Morris does not allude to these, and he has perhaps little occasion to do so in his argument; Professor Sewall mentions them as suppositions of "uncertain fact," and quotes two in illustration of this explanation. I cannot see, after carefully considering his interpretations of these passages, how his doctrine would enable us to distinguish between the cases which require the subjunctive and those which require the present or perfect indicative. Why is *if ever they have fought a battle* any more a "supposition of uncertain fact" than *if these men have fought a battle to-day* (the fact supposed being uncertain)? I fear, however, I have failed to understand this part of Mr. Sewall's paper, for he speaks of ἦν προσμίξωσι (THUC. II. 39) as "in the past, not future." It certainly is not future; but it seems to me impossible to conceive of it as past, or even as strictly present. It rather refers indefinitely to any one of a series or class of

acts; and the Greek is perhaps the only language which ever undertook systematically to distinguish this indefinite "general" supposition by construction from the simple present supposition. Now I hold it to be impossible to bring these conditions under one head with the future suppositions which take the subjunctive by any such sweeping definitions as the one just mentioned. The subjunctive in the latter case is generally interchangeable with the future indicative, and can be translated by this tense in both English and Latin; the other is regularly expressed in both English and Latin by the present indicative, and sometimes takes this form even in Greek. I have already described the quasi-present general condition as a "variation (so to speak) of the ordinary present condition," while the corresponding past general condition is a variation of the ordinary past condition expressed by the past tenses of the indicative.* This important relation, with its consequences, must be apprehended, as it seems to me, before the true force of the subjunctive in protasis can be understood. It will be borne in mind that I refer here to the distinction between the use of the subjunctive in *future* conditions (where it may be either particular or general) and its use in *present general* conditions, and not to the 'general' character which may be given to any class of conditions without essentially changing their nature. A distinction based upon this latter character has been recognized in the subjunctive in protasis by Bäumlein, as I have already stated; † but it led him to no important result, as it is the basis of no distinction in construction. Bäumlein shows in his first statement of the subject that he has no such distinction in mind as that which I have used in my classification; for his

* See *Transactions* for 1873, pp. 64-66.

† See Bäumlein, *Untersuchungen über die griech. Modi*, p. 211. For remarks on his views of this subject, see *Transactions* for 1873, pp. 66, 67. It is there said: "Bäumlein leaves the subjunctive in general conditions, as well as in other kinds of protasis, to be explained on his single principle as denoting a 'Tendenz zur Wirklichkeit'; and he seems to have no suspicion that the two subjunctives stand in different relations to the present indicative." It is no wonder, therefore, that he found his distinction "unessential" (unwesentlich) and "without effect in changing the meaning of the construction" (die Bedeutung dieser Construction nicht ändert). *Modi*, p. 221; cf. p. 224.

very first example is ὁ δὲ κεν κεχολώσεται ὃν κεν ἴκωμαι (II. I. 139), and of eighteen other Homeric examples cited to illustrate the general use of the subjunctive with ὅς κε, "wonach eine Gattung von Fällen als eintretend gesetzt wird," no less than sixteen contain subjunctives referring to the future. It is absolutely necessary to recognize distinctly the element of *time* in order to appreciate the relations of the subjunctive in its two uses, first, to the future and the present indicative, secondly, to the two corresponding uses of the optative. I cannot feel, therefore, that the two systems of classifications which I have discussed in this paper, as they are based on other considerations and exclude wholly or in great part the element of time, can possibly give just prominence to these important relations.

VII.—On Certain Influences of Accent in Latin Iambic Trimeters.

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§ 1. With regard to the influence of accent in poetical composition among the Romans two views have been entertained: the one by Corssen, Weil and Benloew, Lucian Müller, etc., that accent had no influence at all either among the Greeks or among the Romans: the other, by Bentley, G. Hermann, Ritschl, Langen, etc., that the Romans did not disregard accent, but as far as it was convenient, avoided conflict of accent and poetical ictus, because the Latin accent was stress of voice as well as elevation, while the Greek accent, being only elevation as in music, was disregarded. Those who hold the former opinion assert that the coincidence of ictus and accent in Latin poetry is a necessary result of the structure of the verse and the Latin system of accentuation. I formerly was of this opinion myself, but by attempting to prove it to be true, I convinced myself that it was false, and

this paper presents the results of my investigation of the subject. But, although it seems certain that accent had an influence, still this influence is hardly to be attributed to a greater stress than was possessed by the Greek accent. It is, indeed, highly probable, if not certain, that the Latin accent carried with it some little stress of voice, but not appreciably more than the Greek. It is well established that the Greek accent was essentially musical elevation without stress, that is, without conscious effort to produce stress; but the condition of the vocal organs necessary to produce elevation, other things being equal, produces also some intensity of sound, so that an effort would be required to prevent slight stress from accompanying the accent, and there is no reason for believing that such effort was made. In the same way, a consciously made stress of voice would carry with it a slight unconscious elevation. Hence, the Greek accent was elevation with the slight stress which naturally attended it, and ictus among Greeks and Romans was stress with the slight elevation which naturally attended it. This is shown not only from physical considerations, but also by the fact that modern Greek accent, which generally falls on the ancient tone syllable, has the stress-element fully developed; which proves that the germ of stress or the tendency to it already existed in the ancient accent. How early the stress became an appreciable element, it is difficult to ascertain; but it is certain that it must have taken place already in ancient times. Babrius, whose date cannot be determined with certainty, but who, according to Lachmann, must have flourished about the time of Domitian, recognizes the accent to a sufficient extent to show that he felt in it considerable stress or ictus.

§ 2. The following appear to be sufficient reasons for doubting that the Latin accent contained considerably more ictus than the Greek:

1. In the first place, all the Roman grammarians treat accent as a mere elevation of tone, except that Diomedes, who flourished in the latter part of the fourth century, says: "Accentus est elatio orationis vocisve intentio." It is maintained by some that the Roman grammarians blindly followed

the Greeks in this, and that Diomedes alone perceived the truth. It is true that in many grammatical questions the Romans adopted and tried to adapt what the Greeks had done; but in some matters, and especially in this very question of accentuation, they did not imitate the Greeks, as is shown by the fact that the Roman rules give us 'ámant,' 'amántque,' while the Greek rules give *ῥέγει, λέγει τε*; and similarly 'μέᾱ,' 'meǎque'—*λέγε, λέγε τε*; 'mīᾱ,' 'mīᾱque,' *εἶπε, εἶπέ τε*. Further, the language of Diomedes does not necessarily imply that he considered accent to be stress; for the word "intentio" could well be taken from the instrumental nomenclature, where it means 'tightness' of a string and refers to musical pitch; and even supposing that he imitated the Greeks, we may readily believe that this very expression was borrowed from the *ἐπιτεταμένη προσφῶδια* of Glaucus of Samos. Moreover, Diomedes does not call accent "elatio orationis" and "intentio vocis," but "elatio" or "intentio," implying that he meant the same thing by both expressions. But even if none of these explanations are satisfactory, it would affect the main question in no degree if accent did contain stress in the times of Diomedes; for, as was shown above, even the Greek accent had at that late day already commenced to partake of the character of ictus. It is scarcely necessary to allude to the statement of the grammarian Pompeius that "illa syllaba quae accentum habet *plus sonat* quasi ipsa habet maiorem potestatem," for a man to whom is universally attributed "verbosa et puerilis tractandi ratio" can have no authority in such matters; and besides it does not make any difference if an accented syllable did "sound more" in his times. When Quintilian finds the ictus on the penultima of "volucres" and for that reason says that that syllable must be accented, he does not mean that the ictus *is* accent; for anybody could see that the ictus fell on it; but he means that the verse ("pecudés pictaéque volúcre") shows that this syllable is considered long by the poet in this instance, and hence must receive the accent like all long penults. If he had meant that ictus was accent, we should have to understand that in the same line "pecudes" is to be accented on the last syllable.

This remark of Quintilian's, therefore, teaches two important facts: *first*, that accent and ictus were different things; and *secondly*, that accent, as well as ictus, was observed in reading poetry.

2. In the second place, in the old Saturnian Verse, which belonged to an age that was far from being artificial, conflict often takes place between accent and ictus, particularly in the first half of the verse, where it seems rather to have been sought than avoided, as in

consól censór aidilis quí fuit apúd vos,

where "aidilis" could have been placed first, which would not only have removed the conflict, but also would have caused the three offices to appear in their natural order—aedile, consul, censor.

Nor, if accent had been stress, could Plautus and other poets have written such verses as these:

PLAUTUS: Quam id expetivisse opere tam magnó seném.

TERENCE: Dedit, cenávit. gaúdebam. Item alió dié.

PACUVIUS: Sólisque éxortú capéssit cándorem óccasú nigrét.

Yet such verses are very numerous.

Further, we should surely expect the ictus to fall upon the accented syllable where it could be done with no trouble, and this coincidence could be secured at the close of an Iambic trimeter by employing a monosyllable or trisyllable at the end; but in fact the monosyllable at the end was avoided as much in trimeters where it secured agreement, as in hexameters where it caused conflict of ictus and accent; and trisyllables are evidently not sought after.

Again, when Ennius introduced hexameters, he would have avoided monosyllables at the end of verses, and other causes of discord. It may be claimed that he imitated Homer in this, as he certainly did in many other things; but by a careful examination and computation, I have found that he left such matters to take care of themselves, and not till a later day was any effort made to force the ictus to fall upon the accent. A full investigation of this subject would require a long discussion, and belongs to hexameters, while I propose to confine the present question to trimeters.

But I shall call attention to one more illustration of the impossibility of assigning to accent any considerable degree of stress. In the Galliambics of Catullus such verses as the following would have been intolerable :

Phrygium út nemús citáto cupidé pedé tetigít.
Ut apút nivem ét ferárum gelidá stabúla forém.

These verses were, indeed, intended to be wild and fiery ; but still, if the accent had been of the nature of ictus, no one could have felt the rhythm at all.

3. Thirdly, syllables are sometimes contracted in such a way that the accent is removed to another syllable, and the accented syllable itself even vanishes. For instance, in the dramatic authors, the forms of 'súus' and 'túus' (or more properly 'suos,' 'tuos') are very often read as one syllable, the *u* becoming a consonant. Many examples of similar abbreviations of other words occur. So Virgil writes 'arje te,' 'abjete, 'tenvia,' for 'arête,' 'abfete,' 'tenúia.' And very often, especially in derivatives, a vowel which has the acute accent becomes modified in a way which renders it scarcely credible that much stress could have been on it in its modified form, as 'capio,' 'accipio,' 'candeo, accendo,' etc.

§ 3. Having thus shown that the recognition of accent in poetry could not have been due to its having greater stress than the Greek accent, I shall now present in a general way what appears to me to have been the cause of its being recognized at all ; for in the sequel I shall show that it certainly was recognized under certain circumstances.

1. Each word receives its individuality, so to speak, from its accent, by which one syllable, and one only in each word, is distinguished from the rest by a special pronunciation. Hence, it is naturally more agreeable to the ear to hear the ictus on that syllable which is already marked by a distinction, especially as that distinctive peculiarity contains a slight element of ictus, as has been shown. For if any other syllable receives a peculiar pronunciation, the word necessarily sounds unnatural. Now, in Greek the accent occupies so many different positions in words of the same metrical form, that the introduction of a variation from the usual pronuncia-

tion does not seriously mar the form of the word. Moreover, as quantity had to be regarded, it would have been almost impossible to regard accent also, as this stood in so many different relations to quantity. It was even necessary for a Greek to learn the accent of each individual word; and before doing this, he could only assign the accent within certain limits. The Romans, on the contrary, were accustomed to an entirely fixed accent in all words of the same metrical form, and the first time they heard a word uttered, their ears demanded that the accent should fall on a certain syllable. Consequently a word with the ictus on an unaccented syllable sounded much more unnatural to a Roman than to a Greek. Hence, to put it briefly, one reason for avoiding conflict of accent and ictus was *the uniformity of Latin accent*.

2. Again, the laws of accentuation along with the structure of verses often causes the ictus and accent to fall together, whether the poet chooses, or not, to have them together, as in the Iambic trimeter just before and after the caesura, and in the closing feet of the heroic hexameter. This frequent accidental or necessary coincidence of ictus with accent, having become familiar to the ear, caused a verse in which it did not exist, to sound rough and unnatural. Consequently, in those parts of verses where such a coincidence naturally occurred very often, the poets in the course of time sought to make it universal.

3. Finally, the last syllables being uttered very obscurely before the time of Ennius, an attempt was made to keep the ictus off them as modifying the words too much; but to keep the ictus off the final syllable is, in very many instances, to place it upon the accented syllable. This subject will be discussed more at length in another place.

§ 4. I shall now proceed to investigate the relations of ictus to accent in Iambic Trimeters; and first I shall discuss the subject in a general way, and then shall examine individual feet and words.

In order to ascertain whether the Romans regarded accent in poetical composition, we have four tests which we may apply. First, we can compare verses written by ancient

authors with verses which we know to have been composed without any reference to accent; and as it happens that I once wrote some Iambic Trimeters myself, both in Latin and in Greek, I shall make use of them to show that while in the Latin ones certain conflicts of ictus with accent occur which are not to be found in the Latin authors, the Greek ones differ in no respect from the Greek Tragic verse. Whether these verses, as poetry, are bad or execrable, does not matter; they have the *structure* of the ancient verses, except that no attention was paid to accent. The Latin verses will be recognized as a translation from Mark Antony's speech in "Julius Caesar," and the Greek ones as a translation of Cato's soliloquy in Addison's "Cato." (They are appended to this article for reference.) Secondly, we may read Greek verses *with Latin accent*, and see whether the relations of ictus to accent are the same as in Latin verses; but in doing this we must not, like Langen, overlook the fact that words of the various metrical forms do not occur in the same numerical ratio to each other in the two languages. Thirdly, we can examine into the disposition of words in the verses themselves, and see whether or not those conditions are avoided which would produce conflict of ictus with accent. Finally, we may compare with each other the verses of authors who wrote at different periods, and see whether we can trace any progress or modification in the relation of ictus to accent. In the hexameters this can be done very successfully; but in trimeters the want of the works of comic writers after Plautus and Terence renders this mode of investigation less important. I shall apply these various methods indiscriminately, as the circumstances may seem to require.

I have compared the number of those ictuses which come in conflict with accent, with the total number of ictuses, in many different authors. In counting the conflicts, I have not taken into account every instance where an ictus falls on an unaccented syllable, nor where an accent fails to have an ictus fall upon it (in dissyllables and monosyllables); but only those instances where the word has no ictus on the tone syllable, but *at the same time has an ictus on an unaccented*

syllable. Thus, where words of the forms \cup , — , $\cup\cup$, have no ictus, I count it no conflict; and words of the forms $\text{—}\cup\text{—}$, $\text{—}\cup\text{—}$, with two ictuses, one of which falls on the accent, are not regarded as presenting conflicts.

It is a little remarkable that Corssen, in discussing this subject, compares the conflict between ictus and the *Greek* accent in Greek verses with the conflict between ictus and Latin accent in Latin verses, and then goes into an elaborate investigation of relations that exist between the two systems of accentuation, in order to show that the Latin system was better adapted to bring about coincidence of ictus and accent; while he might have dispensed with the whole discussion by applying the Latin accentuation at once to the Greek verses. He even produces Greek verses in which the ictus falls upon the accent in nearly every foot, as,

ὄρθην κελεύεις ἦ τὸ ζένερον φαίνεται;
ἐφ' οἷς αἰεὶ γελῶσιν οἱ θεώμενοι.

How much, then, it would have strengthened his position, if he had observed that this occurs just where the Greek accent happens to conform to the Latin laws! Yet a still more careful inquiry will show that his position is untenable.

In Plautus about the fifth part of the ictuses stand in conflict with the accent, and in Terence a little less, but not quite so few as the sixth part. In Aristophanes, if we apply the Latin accent, we find conflict with about one fourth of the ictuses, and in Aeschylus somewhat more, but not quite so much as a third. From this we could infer at once that conflict was avoided to some extent by Plautus and Terence, unless there be something else which tends to produce coincidence in Latin more than in Greek; and if we adopted Langen's mode of procedure, we should conclude the matter at once, and we shall presently see that our conclusion would indeed be correct, but not because the method of proving it would be correct; for Latin verses are better suited than Greek ones for bringing about coincidence, just as the Greek comedy, not avoiding the incision in the fifth foot ($\cup\text{—}\cup\text{—}\cup$), shows a smaller ratio of conflicts than the tragedy, which,

to some extent avoiding this incision, more frequently presents the ending $\asymp \acute{_} | _ \acute{_}$, where conflict is inevitable.

Nearly all Latin Trimeters have caesura in the third or fourth foot. In order to be brief, I shall examine only the one in the third foot. Now, caesura brings about this form $\acute{_} \asymp |$, which prevents conflict of ictus and accent unless a monosyllable immediately precedes the caesura; and if we inspect the form of a whole verse, $\asymp \acute{_} _ \acute{_} _ \asymp || _ _ _ \acute{_} _ _ _ _$, we shall see that coincidence must take place *after* the caesura in all words of the following forms:

$\acute{_}$	$\acute{_} _$	$\acute{_} _ _$	$_ _ \acute{_} _$
$\acute{_}$	$\acute{_} _$	$\acute{_} _ _$	$_ _ \acute{_} _$
	$\acute{_} _ _$	$\acute{_} _ _ _$	$_ _ \acute{_} _ _$
		$\acute{_} _ _ _$	$_ _ \acute{_} _ _$

Why Corssen counts the molossus ($_ \acute{_} _$) here, I don't understand: perhaps his printer did it for him. It will be observed that words of four syllables must receive two ictuses, which is not to be regarded as a case of conflict as long as one of them falls on an accent. The remaining possible forms for this position are $_ \acute{_} _$, $_ \acute{_} _$; $_ \acute{_} _ _$, $_ \acute{_} _ _$, $_ \acute{_} _ _$, $_ \acute{_} _ _$, in which the ictus would fall as follows: $\acute{_} _ _ _$, $\acute{_} _ _ _$ (which is not tolerated), $\acute{_} _ _ _$ (which with elision of final syllable is very common), $\acute{_} _ _ _$ (which is very common: in fact Ritschl and others hold that in ante-classical times words of the form $_ _ _ _$ had the accent on the first syllable, as 'fámilia,' 'hábu^{er}im': a subject which will be discussed below). Hence we see that only words of these forms $_ _ \asymp$, $_ \acute{_} _ \asymp$, $_ \acute{_} _ _$, $_ \acute{_} _ \asymp$ (?), can cause conflict immediately after the caesura; for words of the forms not already mentioned, viz.: $_ \acute{_} _ _$, $_ _ \acute{_} _$, $_ \acute{_} _ _$, $_ _ \acute{_} _$, $_ \acute{_} _ _$, cannot possibly occupy this place; and words of five or more syllables may be neglected as being of rare occurrence.

Further, as incision in the middle of the verse is very rare ($\asymp \acute{_} _ \acute{_} _ \asymp || \acute{_} | _ \acute{_} _ _ _ _$), it follows that the first *two* ictuses after the caesura will generally coincide with the accent, or at least will not come into conflict with it; for a

word of two syllables creates a caesura in the fourth foot which in its turn prevents conflict in the next word, and a trisyllabic word, $\text{—} \smile \text{—}$, has two ictuses, one of them on the accented syllable. The same happens when a word of four syllables with a long penult follows ($\text{—} \smile \text{—} \smile \text{—}$).

Now let us examine the beginning of the verse. If the verse begins with a monosyllable, the first and second ictuses bear the same relations to the accent that they do after the caesura in the third foot. A trisyllabic word does the same thing, and besides causes the first ictus to coincide with the accent.

From these considerations it becomes evident that harmony of ictus and accent very frequently exists of necessity, or by accident, if we may call it accident. Consequently, the same harmony is tolerably common in Greek verses read with Latin accent, as I have above shown. But how does it happen that it is rarer than in Latin verses? This I shall now attempt to show. In the first place, it has already been proven that just before the caesura there will be coincidence, unless a monosyllable precedes. Now in Greek there are many more monosyllables adapted to occupy this place than in Latin, as μέν, δέ, γάρ, γε, τε, etc. Consequently there are very many such verses as EURIP. *Hel.* 837, 845, 847, 874 (all taken from fifty lines):

ταῦτῳ ξίφει γε· κείσονται δὲ σοῦ πέλας.
τὸ Τρωικὸν γὰρ οὐ κατασχυνῶ κλέος.
ὅστις θέτει μὲν ἐστέρησ' Ἀχιλλέως.
ἦκει πόσις σοι Μενέλεως ὅδ' ἐμφανής.

If any one will examine a few verses of any poet, he can have no doubt about this fact.

Secondly, I have already alluded to the fact that incision in the middle of the verse tends to prevent coincidence. But as nearly all, or at least very many, verses have caesura in the third foot, nothing but a monosyllable can usually stand just before incision in the middle ($\text{—} \smile \text{—} \smile \parallel \text{—} \smile \text{—} \smile \text{—}$). Now, in Greek there are many more monosyllables adapted to begin a new clause, or to follow caesura, than in Latin, the

frequent use of the article alone being sufficient to give Greek the preponderance in this respect, as EURIP. Bacch. 853, 1063:

ἔξω δ' ἐλαύνων τοῦ φρονεῖν ἐνδύσεται.
τοῦντεῦθεν ἤδη τοῦ ξέρον τι θαυμ' ὀρώ.

But such peculiarities of Greek do not account for all the difference between Latin and Greek in this respect, as I shall presently show. It may be asked why I am unwilling to attribute this entire difference to the effort on the part of the Romans to avoid discord between ictus and accent. My only reason is that it is not true that this entire difference is due to that cause; for I have just shown other causes to exist, and whether the causes can be pointed out or not, the *fact* is nevertheless established that coincidence resulting from accident or necessity is much more frequent in Latin than in Greek; for the Latin verses appended to this article exhibit nearly as frequent coincidence as the verses of ancient authors do, while the Greek verses do not differ perceptibly in this respect from the ancient Greek verses. To what then is the more frequent coincidence in the Latin than in the Greek verses to be attributed, as they were both written without the slightest reference to accent? It is true, indeed, that the Latin verses differ from those of the Roman Comedians in having the even feet pure, which increases the chances of accidental harmony; for if in the line,

vidēte quāntam fēcerit Casca invidiūs,

we place a word forming a molossus, say 'formarit,' in the place of 'fecerit,' we shall have discord; but this only affects the comparison of these verses with ancient ones, and does not account for the difference between these Latin verses and the Greek ones I wrote, for I followed exactly the same scheme in the latter, except that I regarded the Porsonic law, which does not account for the whole difference. (Let it not be forgotten that when I speak of harmony or discord in Greek verses, I mean harmony or discord of ictus with *Latin* accent applied to the Greek words.)

From this discussion so far we can only draw the conclusion that harmony of ictus and accent is more frequent in Latin

than in Greek trimeters, but that we should expect this to be the case to some degree, even if accent was entirely disregarded, but hardly to the same degree. Hence it is clear that we can come to no very definite conclusion until we examine individual words, or, as they are sometimes called, word-feet, in their relations to the feet or verse-feet; that is, we are not to compare all the ictuses in Latin verses with all in Greek, but we are to compare words of particular metrical forms in the one with words of the same forms in the other; and from this comparison it will appear that certain sorts of conflict were avoided by the Romans, but that this happens so rarely in comparison with the whole number of ictuses or even of coincidences, that the effect is hardly appreciable in a general comparison. And we shall even find that certain relations of ictus to accent were not allowed, although in the same word it was not demanded that they should coincide, as, for instance, in this verse,

insúltat ín nos próditiŏ truciŏsimá,

the discord in the word 'proditio' is intolerable, while in

insúltat ín nos saéva próditiŏ furéns,

the relation of ictus to accent in that word is quite admissible, and yet neither ictus falls upon the accent, and consequently the number of coincidences in this verse, which is a correct one, is no greater than in the former one, which is incorrect; whence it is evident that a general comparison would not throw any light upon such facts, nor even detect their existence.

§ 4. But before speaking of the individual feet, it will be necessary to look into the form and nature of the verse. I shall regard the Iambic trimeter as a verse consisting of three dipodies, in each of which the stronger ictus falls on the second foot, not on the first. This is distinctly taught by ancient writers, and is accepted by Westphal, Geppert, and others. It does not matter if this verse *was* originally trochaic with anacrusis, thus —|´—|´—|´—|´—, in which the stronger ictuses fell on the first foot of each dipody; for after this verse had been long employed in the drama, and

was declaimed on the stage—not sung, it is well established that they got into a habit of reciting thus, — — ◡ ◡ | — — ◡ ◡ | — — ◡ ◡. This seems to have been the usage already among the Greek Tragedians, since Aeschylus and Sophocles, when a proper name did not otherwise suit the verse, did not hesitate to place a choriambus instead of the first dipody, as *ÆSCH.* Theb. 488, *SOPH.* Frag. 785 Dind. :

Ἰππομέδοντος σχῆμα καὶ μέγας τύπος,
Ἄλφεισίβοιαν ἦν ὁ γεννήσας πατήρ,

which verses clearly cannot be read with anacrusis, unless, indeed, we adopt the view of Dindorf and others that the second syllable of such words was arbitrarily lengthened, which appears to me a rather arbitrary way of removing the difficulty. In other instances Sophocles admits the anapaest,* which I believe Euripides always does, when it is necessary to employ such words. This is not due to a change in the character of the verse, but to the fact that the anapaest was becoming more tolerable, which enabled Euripides to avoid the insertion of the choriambus in place of the first dipody. But whatever may be the true way of reading the verse, the following way, which is very prevalent, is certainly wrong :

— ◡ ◡ — | — ◡ ◡ — | — ◡ ◡ — ;

for the stronger ictus of each dipody must fall on that foot which contains the short* thesis, so that we must either read thus :

— | ◡ ◡ — — | ◡ ◡ — — | ◡ ◡ — — ,

or without anacrusis thus :

— — ◡ ◡ | — — ◡ ◡ | — — ◡ ◡ ;

that is, the greater ictus must fall on the pure foot, whether the verse is treated as being trochaic or iambic. I shall, therefore, first treat it as iambic, with the greater ictus on the

* I follow established usage with regard to the words *thesis* and *arsis*, although the ancients used them in exactly the opposite sense, *thesis* being the syllable that receives the ictus, and meaning the planting of the foot in marching, while *arsis* was the raising of the foot—not the voice.

second foot of each dipody, and then briefly adapt the investigation to the trochaic scheme.

§ 5. I now proceed to the laws themselves which govern the relations of ictus to accent; and first I shall discuss those which relate to words containing long syllables only, then those relating to words containing both long and short syllables, and finally those affecting words which contain only short syllables.

1. (a) In words containing only long syllables we find scarcely any influence of accent in the odd places (first, third, and fifth), which places I shall first discuss. In these places the accent is disregarded, because the ictus is much weaker on these than on the even feet, and does not modify the natural sound of the word as much. This seems to be the proper place to speak of the relative force of the different ictuses. The stronger ictuses are not all of equal stress, and the same is true of the weaker ones. The strongest ictus of all is on the second foot, the next strongest on the fourth, and next on the sixth; and of the weaker ictuses the strongest is on the fifth foot, the next strongest on the third, and the weakest ictus of all on the first. If we represent the stress of each ictus numerically, they will be something like the following:

$$\text{---} \overset{1}{\text{---}} \text{---} \overset{6}{\text{---}} \text{---} \overset{2}{\text{---}} \text{---} \overset{5}{\text{---}} \text{---} \overset{3}{\text{---}} \text{---} \overset{4}{\text{---}},$$

where the numbers merely illustrate, and do not pretend exactly to represent the relative stress. This is because the strongest ictus being on the first dipody, and the force of an ictus depending as much on its ratio to its fellow-ictus as on its own absolute stress, the other ictus in the same dipody would naturally be weakened, and just in proportion as the stronger ictuses are weakened as you proceed in the verse, the weaker ictuses would be strengthened, until the fifth would be nearly as strong as the sixth. A full investigation of this subject would lead us into too long a discussion of music and general phonetics. From the above it is evident that a conflict of ictus with accent would create a maximum discord in the second, and a minimum in the first foot.

(a) It is evident that in the fifth foot the accent is entirely neglected; for there are very many such verses as *PLAUT. Cas. 14*:

huc mihi venísti sponsam præceptúm meám.

(β) In the third foot an influence is claimed for the accent by some able scholars, nor will I deny that there may be some truth in their opinion; but if you examine the scheme of the verse, you will see that the word must have the form $\text{—} \text{—} \text{—}$ or $\text{—} \text{—} \text{—}$, in order that conflict may occur:

$\text{—} \text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} \text{—} \text{—} \text{—},$
 or $\text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} \text{—} \text{—} \text{—};$

but either supposition not only requires the verse to be without penthemimeral caesura, but also divides it in the middle—faults which would, anyhow, prevent the existence of such verses. But if a trisyllable or a longer word in that position suffers elision, the conflict remains, while the caesura is not prevented, and this not unfrequently occurs, as PLAUT. Cas. 326, TER. Hecyr. 412, HOR. Epod. v. 97,

quam id expetivisse opere tám magno seném.

vereor si clámorem eius híc crebro audiát.

• vos turba vícatim hinc et hinc saxís peténs.

Here the conflict is really between the accented thesis of the third foot and the ictus of the second, as the ictus of the third still falls on an accented syllable; but this also is a result of necessity, unless a trisyllable with long penult or a still longer word follows; but a trisyllable cannot follow, because it would create conflict in the fourth foot where a strong ictus falls, thus:

$\text{—} \text{—} \text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} \text{—};$

and verses with a word of four syllables in that position actually occur, as TER. Adelph. iv. 7, 11:

audivi. et dúcenda índotátast. scilicét.

This shows clearly that the conflict in the third was not so much avoided as prevented by the scheme of the verse and the harmony demanded in the fourth foot. This same peculiarity of the scheme (I mean the demanded caesura and the forbidden incision in the middle) even prevents a cretic word ($\text{—} \text{—} \text{—}$) from including the third foot *although there results no conflict*; thus:

$\text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} | \text{—} \text{—} \text{—} \text{—} \text{—} \text{—},$

a form of verse which is extremely rare.

The conflict rendered possible by elision in the middle of the third foot led Hermann (Epit. § 79) to the remarkable conclusion that the long penult, which by elision became the ultimate, was shortened, as 'cóncede huc,' 'sécede huc,' etc. But (in addition to an explanation which will presently be given) such elisions secure caesura, and so are introduced, in spite of the fact that they create a conflict of a certain sort. Even in the few verses which I have appended to this article, one example of this elision occurs :

Signi usque Pompei ipse Caesar maximus.

And I find instances of this among other verses which I have written, but never a conflict (otherwise) in the third foot. Therefore such verses as TER. Andr. 718, etc. :

amicum amátorém, virum in quovis locó,

are faulty only because they have incision in the middle, and lack the proper caesura; for, as to the second foot, the syllable *mā* has sufficient stress to receive ictus, as it is long and is preceded by a short grave syllable. Bentley's views on this subject are as remarkable as Hermann's. He holds (De Met. Ter. *sub fin.*) that the third and fourth feet must not close with the ends of words, because that would cause ictus to fall on final syllables. Now, it is true that ictus, under certain circumstances hereafter to be investigated, must not be placed on the ultima; but in the case before us it is the caesura which prevents the incision in question, and such lines as

persuasit nóx, amór, vinum, ádolescent(en)já,

are faulty because of incision in the middle and absence of proper caesura. It may be argued that the caesura is a result of the other principle, instead of *its* being a result of caesura; but a little reflection will convince any one that caesura was made on its own account, particularly as it was employed by the Greeks who had certainly no objections to placing ictus on a final syllable. It is true, the caesura penthemimeral or hephthemimeral is more uniformly observed in Latin than in Greek, but this results from the more frequent use of the spondee, and in the fourth foot, as I shall show, the discord

caused by ictus on the ultima where the penult is long is avoided.

(γ) Having spoken of the fifth and third feet, I now come to the first. Since the strongest ictus of all falls on the second foot, the first foot, which, so to speak, is under the shadow of the second ictus, is subject to less stringent laws than any other foot in the verse. Accordingly we find in this foot synzesis, ecthlipsis, conflict of ictus with accent, and all licences more frequently than anywhere else. I shall produce a few illustrations taken from TER. Phorm. :

addo istúc : inprúdens tímuit ádulescens : sínó.
non fuit necesse habere : set íd quod lex iubé
nempe Phórmíonem. D. istunc patrónum mulierís.
ego deós penátis hinc salútatum domúm.
senectútem oblétet : respice aétatem tuám.

But the fact is a well known one, and there is no need of further illustrations. Even such forms as 'vilicus' (— ∪ ∪), 'filius' (— ∪ ∪), 'hominem' (— ∪ ∪), which involve a conflict of ictus and accent scarcely tolerated elsewhere, sometimes occupy the first place.

(b) I shall now examine the even places. In them, on account of the stronger ictus, the discord is greater when ictus conflicts with accent.

(a) Of course, as the sixth foot is always an iambus, and we are now discussing words which contain only long syllables, we pass this foot by for the present. It may be asked *why* this foot is always an iambus. To this it is sufficient to reply that the penultimate foot of nearly every kind of verse was, for reasons which it is unnecessary to mention here, kept pure, and that the Iambic trimeter was originally trochaic with anacrusis and catalexis :

— ∪ — — | — ∪ — — | — ∪ — — ∪,

so that the sixth iambus results from the original fifth trochee.

(β) In the fourth foot, where the next to the strongest ictus of all is found, conflict is very rarely tolerated ; yet examples are not entirely wanting, as PLAUT. Cas. 322 :

meis inimícis voluptatém creaverím,

where it will be observed, by the way, that the greater ictus

of the fourth foot excuses the loose pronunciation in the other foot of the dipody where *volup-* appears as one syllable *v'lup-*.

Further examples are:

ENNIUS: *Palam mutíre plebeió piaculum ést.*

PLAUT. *Mil. II. 6, 22: nisi mihi supplícium virgarúm de te datúr.*

LIV. *ANDR. 40 (R.): puerarum mánibus confectúm pulcerrímé.*

TER. *And. Prol. 7: veteris poétæ maledictís respondeánt.*

But the instances are very rare compared with what would naturally take place, so that we must suppose that this conflict was, for the most part, avoided. Nor is it difficult to see why it should be. The Roman poets were accustomed to the Greek verses, which never admit the spondee in the fourth place; consequently when they introduced it themselves, they still shortened the thesis as much as the nature of words would admit, that is, they made the thesis "irrational." Besides, as has been shown, the difference between arsis and thesis must be as great as possible in the even feet, to prevent the verse from losing its iambic character. Moreover, whenever this discord occurs, the ictus falls upon the ultima and at the same time on the antepenult if the word has more than two syllables, which must be the case to prevent incision in the middle. Now, it is true, the ictus on the antepenult would not, of itself, be objectionable, as that syllable is generally a stem-syllable, as in 'plébeió,' or a prefix requiring emphasis, as in 'cónfectúm'; but when an ictus falls also on the ultima, so that the accent lies between two ictuses, on a syllable which must be to a great extent shortened and rendered obscure, the natural sound of the word is too much altered; and especially was this the case in the times of Plautus when the last syllables were very obscure—so much so that the Latin language was in danger of losing its terminations to a great extent, as actually occurred in the case of its sister languages, the Oscan and Umbrian: a danger from which it was rescued by its contact with Greek, and especially through the influence of Ennius, who reduced to fixed laws many things which he found entirely unsettled. Nearly all final syllables, for instance, had been treated as short. Some of these, though originally and properly long, he left short, as in 'beně,' 'malě'; others he left common,

as in 'miliĩ,' 'tibiĩ,' 'ubiĩ,' but the most of this class he restored to their legitimate quantity, as in 'domĩ,' 'malō,' 'metũ,' 'brevĩ,' 'veliĩ,' 'maxumē.' This obscure sound of the final syllable is shown, further, to have existed by certain forms of the language, especially as they are found in inscriptions of an early date. In the ordinary forms a relatively obscure pronunciation is shown by a comparison of the forms *datas* (Sanskrit), *δορός*, *datas*. Consequently, if the ultima is elided, the discord is to a great extent removed, even if the ictus falls on the antepenult—not, as Hermann says, because the penult (thus rendered ultimate) is shortened, as "ácced' húc," but because, as shown above, the syllable on which the ictus falls is always one adapted to receive stress, and the ictus which follows falls either on a stem-syllable or on a prefix demanding stress. Cases of this sort are much more frequent than those where no elision takes place, and to attribute this to chance is simply to believe that the majority of words suffer elision. I give here some examples of this elision :

PLAUT. CAS. 468 : si neget adesse exanimatum ámittat domúm.

" " 470 : sed uxorem ante aêdes éccillam (?). hef misero mihi.

" " 496 : tuam arcessitúram esse úxorem meám.

" " 497 : ergo arcessivisse ait sese ét dixisse té.

TURPIL. 90 (R.) : propter peccátum paúsillum indignissimĩ.

ENN. 242 (R.) : Asta atque Athénas, ánticum, ópulentum oppidúm.

LIV. ANDR. 14 (R.) : procat, tolerátis témploque hánc deduciis.

TER. PHORM. II. 3, 76 : praeterierát iam ad dúcendum aétas. omnia haéc.

" " IV. 1, 7 : quaeso igitur cómmorábare úbi id audiverás.

" " " 11 : quid gnato optigerit me absente, audistin, Chreme.

Many more could be produced, but these suffice to illustrate.

(γ) In the second foot the law is most stringent, but even here exceptions can be found, as PLAUT. CAS. 402 (unless the verse is corrupt) :

quinque hanc omném rem meae erae iám faciam palám.

If the ictus falls before the accent, the discord does not belong to this foot, as I have already shown, but to the third where the weak ictus on the ultimate is tolerated, but is rare for reasons already explained, that have no reference to accent.

An example is TER. HEAUT. TIM. 147 :

decrevi tántispér me mínus incuriaé.

Here the stem-syllable 'tan-' is well suited for ictus, and as the ear is accustomed to the word 'tantis,' the conflict with the accent in 'tantisper' gives no offense. But even if there were any discord, it would belong to the third foot; for the law merely excludes the second ictus from the ultima, when the penult is long. Hence it may even happen that the thesis of the second foot be an accented syllable, as Heaut. Tim. Prol. 11; NAEV. 37 (R.):

orátorem esse voluit mé non prologúm.

dic quó pacto eúm potiti: púgnan an doló.

This, however, is rare, and was evidently avoided to a considerable extent. In PLAUT. Cas., as far as I know, there is no instance of it except when a monosyllable forms the thesis, as v. 333 (Gep.):

sine módo rus véniat ego remíttam ad te virúm.

Since in the second foot the ictus must not fall on the ultima when the penult is long, it follows that if a monosyllable immediately precedes the caesura, another monosyllable must precede that one, or else elision must take place before it, thus:

— — — | ' — || — ∪ ' — — ∪ ' ,
or — — — ' , | — || — ∪ ' — — ∪ ' .

This use of two monosyllables is usually attributed to an attempt to place the second ictus on an accented syllable to prevent the discord that would result from ictus on an unaccented syllable followed by an accented monosyllable without ictus. This is rather absurd, for the former of the monosyllables is written first. Poets don't begin at the caesura and build the verse backwards. The true explanation is this. When the Latin poets admit the spondee into the second place the second ictus must not fall on the ultima. One way of avoiding this is to have caesura in the second foot:

— — — | — — || — ∪ — — — ∪ — ;

but as often as this happens the space between this and the main caesura must be filled by either a dissyllable or two monosyllables, and of course the latter would frequently

happen to be more convenient. The second monosyllable is in another dipody, and indeed in the third foot where the accent is neglected, as I have already shown. A few illustrations will make this clear :

PLAUT. CAS. 51 : nunc ne tu | té mihi || respondére postulés.

" " 231 : sic tandem | sí tu || Iuppitér sis mortuós.

" " 36 : post autem | rúri aut || ervom nísi comederís.

That the monosyllable before the caesura has nothing to do with it, is clearly shown by verses which have the iambus in the second place, for then the ictus *does* fall on the ultima when that monosyllable follows, as Cas. 3 :

quid tu, malúm, me sequere ?—Quí certumst mihí.

Now if the discord were between *-lum* and *me*, it would be as great in case of an iambus as in case of a spondee in the second foot. Of course, when a monosyllable is under the second ictus, another monosyllable will not necessarily follow if the poet chooses to employ the hephthemimeral caesura ; hence we frequently meet with such verses as Cas. 12, 13, etc.:

quin potius | quód legatunst || tibi negotiúm.

id curas | áque urbanis || rébus abstínés.

In Greek verses the caesura, as has already been shown, is often preceded by a monosyllable, and the result of this is that conflict frequently takes place in the second foot between ictus and (Latin) accent. I mentioned this as one of the reasons why such conflicts would naturally be more numerous in Greek than in Latin ; but as I have just now shown that this conflict was really avoided in Latin, it may appear as a contradiction ; but a little reflection will convince any one that this is not the case. The number of such verses is so small in Latin compared to the whole number of ictuses, or to the number of such verses in Greek, that in a general enumeration of conflicts, they would produce no appreciable effect, even if the law forbidding the conflict were neglected.

Let us now suppose the verse to be trochaic in its movement. Even with supposition, the laws relating to conflict would naturally be about the same. It is true, the main ictus would then fall in the odd places thus :

— | / — — | / — — | / — — ;

but the thesis is no longer the syllable preceding, but the one succeeding the arsis, and the conflict between the ictus of the first foot and the thesis of second (iambic) foot is the one to be avoided (as this now becomes the weakest thesis of all), which would bring about most of the results examined. Still, the loose manner in which the first ictus is handled would convince me that the verse is to be considered iambic even if it were not known to be so from other considerations.

2. In words containing both long and short syllables, conflict is scarcely avoided at all, so long as the ictus is on a long syllable, and the accent on a short one. Accordingly, a word ending in an iambus receives the ictus very readily on the ultima, $\text{—} \text{—}$. If the whole word is an iambus, of course conflict is inevitable, if it receives an ictus at all; for the ictus cannot fall on the penult, $\text{—} \text{—}$, where the accent is. Corssen asserts that there is as much discord in $\text{—} \text{—}$ as there is in $\text{—} \text{—}$; but he ignores the fact that quantity determines stress in Latin, and that the ictus in $\text{—} \text{—}$ would have to be stronger than in $\text{—} \text{—}$ to give the foot its iambic character. That the second syllable of $\text{—} \text{—}$ is a much more important part of the word than the second syllable of $\text{—} \text{—}$, is shown by the fact that iambic words in the more polished hexameters do not suffer elision, and sometimes even admit hiatus after them.

With regard to the relations admissible between ictus and accent in words which end in a trochee or a dactyl, there has been much discussion among scholars, and appears still to be great diversity of opinion; but the conclusions I came to after an independent investigation made without any bias (as I did not at the time know the subject had ever been discussed), I have no reasons to change, after having read a good deal on the subject. To examine the question would require too much space; but I shall submit my views briefly. In my opinion, the ictus was for the most part excluded from the short syllables (ultima and penult) of such words, and especially from the ultima of dactylic words, but accent was not always (if ever) the sole cause of this restriction. In trochaic words, if the penult is *circumflexed* (which Corssen, *Quintilian*,

and others affirm, and Langen denies), that is, closes with the low tone, the discord caused by ictus on the ultima is not very great, as an unaccented mora falls between the accent proper (the acute part of the circumflex) and the ictus. The chief objection, then, to this ictus is not its conflict with accent, but the stress it gives to a final short syllable, which must have been very considerable, so as to exceed the natural stress of the long penult. But even this objection is to some extent removed by the fact that the ultima forms only half the arsis, and is quickly passed over, while the other half will nearly always be found on a monosyllable or the accented penult of a dissyllable; and it will presently be shown that the second mora of a resolved arsis is better adapted for the accent than the first mora is. This linking together of two words by a resolved arsis, as in

anguis in impluvium decidit de tegulis,

is hardly tolerable unless the words are closely connected in construction, and the second one is a small one, so that the accent may fall under the second mora of the arsis; and at the same time the first one is usually small so as to bear prefixing. Examples seem to be more numerous in trochaic than in iambic verses; but in many of the examples sometimes produced, the ultima is really long, and fills up the whole arsis, as PLAUT. Trin. 714: 'sine dotē (*dotei*) neque,' etc.; Capt. 914, 'carnī;' Mil. 707, 'mortē;' Men. 478, 'partē' or 'partī': cf. ENN. Ann. 420 'sub montē' (Bücheler, Lat. Decl., p. 50).

The unpleasant effect of an ictus on the ultima of a dactylic word has nothing at all to do with accent, for another ictus falls upon the accented syllable: $\acute{\text{—}} \text{—} \text{—}$; but is due, *first*, to the fact that this ictus gives the very obscure ultima too much stress, and, *secondly*, to the fact that the prefixing of so long a word to a shorter one is disagreeable. Consequently this ictus is rare in Greek as well as Latin. Moreover, although in Greek a word which forms a *first paeon* not unfrequently has ictus on the penult $\text{—} \text{—} \text{—} \text{—}$, as ARIST. Birds, 657:

καὶ Μανόῳρῃ λαμβάνετε τὰ σώματα,

still, this ictus is extremely rare if the ultima is elided so that the arsis belongs to two words. In the *Birds* there is one instance of this, v. 1123:

εἰ μὴ παρέξει τὰ μπόρι' ἀνεφγμένα,

where even long words are combined. There is in the *Birds* also one example of ictus on the ultima of an ordinary dactylic word, v. 202:

ἔνυρὶ γὰρ ἰσβᾶς αὐτίκα μάλ' ἐς τὴν λόχμην,

where *μάλα*, being placed after *αὐτίκα*, needs to be forced into close connection with it; and even in the previous verse cited, the elision (which can be shown to have been total in Greek) already unites the words closely, which can never happen in Latin, as the elided syllable in that language was not entirely suppressed.

Ictus on the penult of a dactylic word, if very weak as in the first foot, is sometimes allowed, as in *PLAUT. CAS.*, 'vilicus,' and in the prologue (not by Plautus) 'filius.'

In words which form an antibacchius, the conflict or rather the discord is greater than in trochaic words, when the ictus falls on the ultima, $\text{—} \text{—} \text{—}$; for, *first*, another ictus must also immediately precede the accent, and, *secondly*, the word is longer and more cumbersome, so to speak, and so is less adapted to being closely connected with a succeeding word. In fact, the second reason alone is quite sufficient to account for the exclusion of ictus from the ultima of such words; for the same law is observed in Greek verses. In the *Birds* there is not a single example of this ictus. To illustrate the relative usage in Latin and Greek verses with regard to the remaining forms just discussed, I here present a comparative view of the *Birds* and *Casina*. In the *Birds*, which contains nine hundred and thirty Iambic trimeters, we find the following: $\text{—} \text{—} \text{—}$, 2; $\text{—} \text{—} \text{—}$, 1; $\text{—} \text{—} \text{—}$, 7; $\text{—} \text{—} \text{—}$, 12; $\text{—} \text{—} \text{—}$ (—), 1. The form $\text{—} \text{—} \text{—}$ in both examples represents a word closely connected with the following word: v. 1465, οἷσί τε ($\text{—} \text{—} \text{—}$, —) which is hardly a fair example at all; v. 1693, ἀλλὰ γαμικὴν κτέ. ($\text{—} \text{—} \text{—} | \text{—} \text{—} \text{—}$). In *Casina*, where there are two hundred and eighty-six Iambic trimeters,

we find — ˘ ˘, 1 (in the first foot of v. 635, 'vilfcus'); — ˘, — ˘ ˘, — ˘ ˘ ˘, none at all. In the prologue — ˘ ˘, 1, in the first foot, 'filfus.' In TER. PHORM., where there are six hundred and seventeen Iambic trimeters, we find — ˘, 3, all in the first foot; — ˘ ˘, — ˘ ˘, — ˘ ˘ ˘, none at all. These statistics demonstrate clearly that such ictuses were avoided in Latin; for words of these forms are not so much rarer in Latin than in Greek, and accident favors these ictuses no more in Greek than in Latin. Still, I have found in other plays of Plautus a few examples of most of these ictuses, unless the verses are corrupt: a point which I had no means of investigating. But the whole investigation leads to the conclusion that the ictus on the short ultima of long words was avoided more on account of the undue stress thus given to the syllable and close union of long words with other words, than on account of discord between ictus and accent.

The form — ˘ ˘ ˘ will be treated along with ˘ ˘ ˘.

3. The law affecting words composed of short syllables is this: *The ictus must not fall on the syllable which immediately follows the accent, but may fall with perfect freedom on the syllable preceding the accent*; for the slight elevation of voice which accompanies ictus is extended to the second mora of the arsis and so includes the accent; so that this ictus really harmonizes better with the accent than an ictus does which falls on the accent and extends itself to the next syllable. The ultima, for reasons already given and not pertaining to accent, is not disposed to receive the ictus.

a. (α) An ictus on the ultima of a word forming a pyrrhic (˘ ˘) does not produce much discord, for these words are often almost without accent, and sometimes are employed as monosyllables, and can readily be connected closely with a word following. Hence we need not be surprised to find instances like 'suós amor' (˘ ˘ ˘) in PLAUT. Pseud. 206; for this word frequently suffers synizesis, 'sūōs,' from which it is evident that the word may either be deprived of accent entirely, or else the accent may be removed to the second syllable. But as the words themselves are not very numerous,

and there is still some aversion to arsis divided between two words, and the form \cup , $\cup \cup$ for other reasons is better adapted to the verse, the examples of $\cup \cup$ are rare. They are also rare in Greek verses where an influence of accent is out of the question. In the *Birds* I find only four examples, as v. 71, ὅτε περ, v. 76, ὅτε μὲν, etc., where the words are very closely connected, or the second is even an enclitic. In 'Casina' there is one example, found in the first foot of v. 460:

homínem amatórem ullum, ad forúm procederé,

where a tribrach has become a pyrrhic by elision. In *TER. Phorm.* there *seems* to be one example, iv. 3, 49:

set mi opus erát, (ut aperte tibi nunc fabulér);

but who can believe that the ultima of 'erat' was combined in arsis with 'ut' across the pause and parenthesis? It is clear that 'erát' is here an iambus, the parenthesis and pause justifying the retention of the original quantity of the ending -at. The next foot is, then, an anapaest.

(β) The fact that the ictus very rarely falls on the ultima of a word forming a tribrach ($\cup \cup \cup$), is not to be attributed to any influence of accent, for the accented syllable forms the second mora of the preceding arsis, so that there is no conflict: — $\cup \cup \cup$ | $\cup \cup$; but it is rare because it forces two consecutive arses to be resolved, thus: $\cup \cup \cup$ | $\cup \cup$ thus prefixing *two* words, as it were, to a third one, whereas we have seen that *one* long word so prefixed is unpleasant. Here, again, the Greek usage disproves the influence of accent; for in the *Birds* there is not a single example. In trochaic verses this resolved arsis does not force the resolution of another, and so some examples of it are found: \cup , $\cup \cup$ | $\cup \cup \cup$ (but hardly \cup , $\cup \cup$ | $\cup \cup \cup$). In iambics, therefore, it can only occur when we have a proceleusmatic | $\cup \cup \cup$ $\cup \cup$ | —, and examples of this may be produced, especially in the first foot.

(γ) A word forming a proceleusmatic ($\cup \cup \cup \cup$) very rarely receives the ictus on the ultima, because, *first*, it unites a long word closely with another; *secondly*, it imparts too much stress to a weak ultima; and *finally*, so many consecu-

tive short syllables are unpleasant. Hence we have either $\overset{\frown}{\text{—}} \text{—} \text{—}$ (ultima long), or $\overset{\frown}{\text{—}} \text{—} (\text{—})$, — (ultima elided). (Cf. Cas. 44 (Gepp.) and Men. 361).

b. (a) The general law I stated, relating to influence of accent, forbids ictus to fall on the penult of a word forming a tribrach: $\text{—} \text{—} \text{—}$, that is, $\text{—} \overset{\frown}{\text{—}} \text{—}$. But some examples, especially in the first foot, can be found. The only example I have seen in an even foot is in the fourth foot of PLAUT. Men. 876 :

qui vi me cōgunt ut validus insaniām :

a verse which some have tried to amend, and whose authority Ritschl for some reason rejects, although Corssen says he recognizes it (Aussprach., Vokal., Beton., II. p. 994).

In Casina, while $\overset{\frown}{\text{—}} \text{—}$ occurs thirty times and $\overset{\frown}{\text{—}} \text{—}$ thirty-three times, $\text{—} \overset{\frown}{\text{—}}$ does not occur once, and $\text{—} (\text{—})$ only once, and that in the first foot. In the Birds of Aristophanes, $\text{—} \overset{\frown}{\text{—}}$ occurs thirty times, and $\overset{\frown}{\text{—}} \text{—}$ sixty-four times; or, to omit the form $\text{—} \text{—} \text{—}$, we have $\text{—} \overset{\frown}{\text{—}}$ just as frequent as $\overset{\frown}{\text{—}} \text{—}$. That is, we have the following relations :

Latin	{	$\overset{\frown}{\text{—}} \text{—}$	-	-	-	-	30
		$\text{—} \overset{\frown}{\text{—}}$	-	-	-	-	0
Greek	{	$\overset{\frown}{\text{—}} \text{—}$	-	-	-	-	30 more or less.
		$\text{—} \overset{\frown}{\text{—}}$	-	-	-	-	30

Of the thirty examples of $\text{—} \overset{\frown}{\text{—}}$, twelve, indeed, immediately follow incision in the middle of verses, where they cannot occur so frequently as in Latin verses, as has been shown. But to omit these twelve entirely, the remaining eighteen are nearly all in the second foot, where the scheme of the verse, though hardly so favorable as the scheme of Greek verses, still is not so unfavorable to this ictus as to account for the vast difference of usage. Hence, we must conclude that this conflict between ictus and accent was carefully avoided by the Roman poets. To this conclusion the objection may be made, and has been made, that such words as 'meāque,' 'egōne,' which have the accent on the penult, have the ictus on the penult just as rarely as other ordinary words which form a

tribrach. This fact we must admit; but it can be explained without invalidating the law I stated. Nor is it necessary to deny that these words were accented on the penult, as some distinguished scholars have done. Langen says that Diomedes was the first to attribute such an influence to enclitics in Latin, and thinks that he and others who succeeded him imitated the Greeks in this matter. But, in the *first* place, we can hardly believe that such men as Priscian were so deaf or stupid as to be unable to recognize the accent of their own words. *Secondly*, they could not have imitated the Greeks in this view, because the Greek enclitics did not produce this effect, as will appear from a comparison of 'méa,' 'meāque,' with ῥόνυ, ῥόνυ τε. *Thirdly*, Diomedes seems to have been the first and only one to attribute "*intentio*" to accent, and Langen accepts this as authority for stress, although Diomedes could have taken the expression from Greek writers, and that too without its meaning stress. Now, if Langen is willing to believe Diomedes in this, why does he not believe him and others in another statement, which is of itself probable and cannot be borrowed from the Greeks? *Finally*, it may be admitted that Diomedes was right, but claimed that at his time the accent in such words had shifted its position; that the classic 'méaque' had become 'meāque.' To this we reply: In the space of time that elapsed between Terence and Diomedes it is not probable that the accent should have changed its position in that way, for the accent was more tenacious of its position than of its nature—a fact which we cannot doubt when we consider that even in modern languages derived from it, the accent is, for the most part, still on the Latin tone-syllable, and the same is true of modern Greek compared with ancient, and all admit that the Greek accent has changed its nature, and I think I have established the same in regard to the Latin accent. But Langen denies that accent could have changed its nature so as to possess "*intentio*" at the time of Diomedes, unless it had it already at the time of Terence. How, then, could it have changed its place, especially as in so doing it would have left its customary and legitimate position (◡ ◡ ◡) and taken up an unusual and

strange one (◡ ◡ ◡) hitherto unknown to the language? A further objection is based on the fact that some of the grammarians assert that 'atque' has no accent, from which the conclusion is drawn that '-que' had not the power of imparting a new accent to a word. But if we accept the statement of the grammarians about 'atque,' why not accept their statement about 'meāque'? Moreover, their statement about 'atque' shows that they were not blindly, or rather deafly, following the Greeks, for the Greeks, in similar combinations, employ the accent, as in εἶγε, οὐτίς, ὅδε, ὥσπερ, etc.

I am aware that these arguments do not establish beyond a doubt that such words were accented on the penult; but if they were not, the exclusion of the ictus from that syllable is all the more easily explained, or rather needs no explanation at all. But I have attempted to prove that the accent was on the penult of such words, merely because I believe it is true; and I can explain the exclusion of the ictus from that syllable without resorting to the arbitrary assertion that in this one particular the Latin grammarians did not know what they were talking about.

Assuming, then, that the accent was on the penult, 'meāque,' we offer the following explanation: *First*, the Romans avoided conflict with accent, chiefly because of the uniformity and fixedness of the position of the tone-syllable; but in the words under consideration, the normal position is departed from, so that the main cause of avoiding conflict is removed. *Secondly*, since the great majority of words forming a tribrach (◡ ◡ ◡) received the ictus on the first syllable, the mere force of habit would cause the poets to treat *all* in the same way. But, *lastly*, there is really a worse conflict when the ictus falls on the accent of such words, than when it falls on the first syllable. For, when on the penult, it includes the ultima, which is always a low-toned enclitic, whilst, when placed on the first syllable, it imparts due stress to the stem-syllable, and carries over some elevation to the tone-syllable, as it is the second mora of the arsis.

But examples of ictus on the penult of such words are not entirely wanting in Plautus and Terence (Langen, Philol. xxxi.), and other dramatic writers, as CAECIL. 232 (R.):

egone quid dicam ? quid velim ? quae tu omnia.

(β) Ictus is not tolerated on the penult of a word forming a proceleusmatic (— — — —). Not only is there an intolerable discord between ictus and accent, but also this ictus presupposes the close connection of the word with the one preceding, by means of resolved arsis, thus: — — — — | — — — —, and forces five short syllables to come consecutively. The ancients, both Greeks and Romans, were accordingly in the habit of causing the ultima of such words to be lengthened by inflection or position, or to be removed by elision before a long vowel: — — — — —, or — — — — (—), —. Consequently, if I mistake not, there is not a single instance of — — — — in the Birds—certainly none in *Casina* or *Phormio*.

c. Now we come to a difficult question. According to Langen, words of four short syllables in Plautus have the ictus eleven times on the *first* syllable to once on the *second*—in Terence seven times to once; that is, we have the following relations:

In PLAUTUS, — — — — : — — — — = 11 : 1;

“ TERENCE, — — — — : — — — — = 7 : 1.

Hence, — — — — in Pl.: — — — — in Ter. = 11 : 7.

Bentley, Ritschl, Langen, and others hold that such words in the times of Plautus had the accent on the fourth syllable from the end, but that at the time when Terence wrote, this accent had already begun to vacillate between that syllable and the antepenult. Now, Georg Curtius accepts, or at least formerly accepted, the theory of Langen and others that the Latin accent had more stress than the Greek, and was something between the Greek and modern accent; and if this theory be correct, then it must be true that the words under consideration were accented on the fourth syllable from the end. But then, one of the most important arguments employed by Curtius to establish the existence at one time of a Graeco-Italian language, is the fact that in both Latin and Greek the accent is confined to the last three syllables, whilst in Sanskrit, for instance, this is not the case. Now, either this argument must be abandoned, or else the stress claimed by Langen must be considered erroneous; and which we are to do, I don't see

how there can be any doubt; for it is incredible that between the age of Terence and that of Cicero the accent should have changed so much that Cicero could assert that it was a "*law of nature*" that the accent should never fall farther to the left than the antepenult (Orat. 18, 58); and it seems the more incredible when we consider that the plays of Plautus and Terence were always of great authority and influence, and by being frequently brought upon the stage were kept alive, and so did much to preserve and transmit the language under the form in which they employed it. Yet, Cicero's remark shows plainly that an accent on the fourth syllable from the end was entirely unheard of in his day.

So we may with confidence assume that such words were, like all other words with short penults, accented on the antepenult; and remembering that the accent was chiefly mere elevation of tone, we shall be able to find a satisfactory explanation of the tendency of ictus to fall on the syllable before the accent.

(α) First let us compare the Greek usage. In the *Birds* there are seventy-six words of this sort, of which forty-eight have the ictus on the first syllable, and twenty-eight on the second: $\cup \cup \cup \cup$, 48; $\cup \cup \cup \cup$, 28. Of these twenty-eight examples, twelve occur immediately after incision in the middle of the verse, which incision we have already seen to be, for other reasons, less frequent in Latin than in Greek. Hence we shall not be far from the truth if we assume that in the verses of Aristophanes modified to suit the Latin scheme, there would be two ictuses on the first syllable to one on the second: $\cup \cup \cup \cup$: $\cup \cup \cup \cup$ = 2:1. Most of the examples of $\cup \cup \cup \cup$ occur in the second foot, where they can readily occur in Latin as in Greek verses, for the scheme $\cup \cup \cup \cup$ does not interfere with caesura. Therefore Plautus has about five times, and Terence about three times as many ictuses on the first syllable as Aristophanes. This could not happen by chance.

(β) The causes of this seem to me to be the following: *First*, the elevation of voice, which accompanies ictus, extends itself to the second mora of the arsis, $\cup \cup \cup \cup$; and as the

accent was elevation without any considerable stress, it harmonizes very well with the element of ictus on this second mora of the arsis. But when the ictus fall on the accented syllable, $\smile \frown \smile$, it extends itself to the syllable following the accent, which is objectionable; so that, in fact, the ictus on the syllable preceding the tone-syllable harmonizes with the accent better than an ictus on the tone-syllable itself.

Secondly, in all these words the first syllable is either the root, as in 'habuerit,' 'familia' (cf. 'famulus'), or a prefix which distinguishes compounds from each other, or indicates their relations to their primitives, as in 'abierit,' 'redierit'; 'inhibeo,' 'diribeo'; wherefore these syllables, especially as they are short and unaccented, were pronounced with a certain stress, in order that they might retain their significance. As the modern accent includes stress, it is employed for this purpose, and a secondary accent remains on the primitive word in compounds, as in 'unwahrscheinlich,' 'abgeschrieben'; and so on the root of derivatives, with secondary accent farther on, as in 'obrigkeitlich,' 'Kleinigkeiten.' Now, the Latin accent, being elevation, could immediately follow this stress, and the stress itself was more of the nature of ictus than of (Latin) accent; so that those two syllables were exactly suited for the reception respectively of ictus and accent. There is reason for believing that those prefixes, even when they were long, received stress in the early periods of the language, and it is upon this very theory that Curtius bases his explanation of certain vowel-changes, such as are seen in 'facio,' 'inficio,' 'perficio'; 'scando,' 'ascendo,' 'descendo'; 'cado,' 'ceidi' ('ce-', which is a remnant of the root repeated, denotes completed action and so demands emphasis). Changes like 'ago,' 'egi'; 'facio,' 'feci,' do not invalidate this argument, for they are either contractions from earlier reduplicated forms, or else of a different nature, involving the lengthening of the vowel, something like $\lambda\epsilon\pi$ -, $\lambda\epsilon\lambda\omicron\epsilon\pi$ -. Corssen, in order to explain those vowel changes, assumes that in very early times the fourth syllable from the end could be accented, and that a long penult did not require the accent to fall on it. But this will not explain all the changes; and even if they

were so to be explained, we are by no means to suppose that those accents continued to the times of Plautus; and Corssen himself warns us against understanding him to mean this.

The fact that Terence placed the ictus on the antepenult of such words more frequently than Plautus, may be due to several causes. *First*, the stress on the root or prefix was, of course, a relative thing, because to impart a similar stress to the remaining syllable would be to deprive the stress on the root of its effect. Now this very thing happened, when Ennius introduced the hexameter, and restored the Latin endings, or rescued them from destruction by allowing them quantity and ictus in his great epics, which, for ages, were standard authority. In the times of Terence, the endings, therefore, were not so weak and obscure as in the times of Plautus, which is the same as saying that the roots were relatively weaker. But in the times of Terence, that emphasis on roots and prefixes had not yet vanished, nor even in the times of Virgil had it entirely disappeared, for he employs 'tenuia,' 'ariete,' etc., as dactyls, changing the accented vowel almost into a consonant; for surely no one will claim that such words even as late as Virgil still had the accent on the fourth syllable from the end. Nevertheless, it must be conceded that Virgil did not adhere closely to popular pronunciation, and that words like those mentioned could not in any other way be forced into a dactylic hexameter.

Secondly; even if the change of stress on root-syllables could not have taken place between Plautus and Terence, the difference of usage between the two is not so great but that it could result from a difference of individual inclination or local usage, particularly when we remember that the ictus $\cup \cup \cup \cup$ was allowed even by Plautus. In a similar manner Euripides indulges in what are termed licenses, which Aeschylus and Sophocles, *writing at the same time*, avoided; and I know men who pronounce whole classes of words differently from most people. Who has not heard people say 'governmĕnt,' 'complimĕnt' (noun), etc.? And I have in mind a distinguished scholar and scientist who gives the first syllable of many words such an emphasis that many persons declare he

accents them, as '*Virginia*,' '*congratulate*.' Now, remembering how very different were the earlier associations of the African and the Umbrian, we should expect differences in speaking.

Finally, the slight difference of usage between Plautus and Terence could happen by chance, although the difference between their usage and that of Greek poets is entirely too great to be accounted for in this way. The total number of ictuses on the antepenult (— — —) is very small, and when we are dealing with things so rare, not much importance is to be attached to a comparison of numbers. To illustrate this, let us assume a case. Let us suppose two Latin poems to have been discovered, and the question to have arisen whether they were both from the same author. In one, which contains, let us say nine hundred and eight Heroic hexameters, there are ten examples of conflict between ictus and accent in the sixth foot, and six such examples in the fifth foot; in the other, which contains, say, eight hundred and eighteen verses, there are only three examples of conflict in the sixth foot, and none at all in the fifth. If we attribute importance to so wide a difference, where, however, all the figures are small, we should decide at once that the poems were from different authors; but the poems I have described are the tenth and the ninth books of Virgil's *Aeneid*. At the opening of the discussion of this question, I assumed the data furnished by Langen and compared the ratio between the two sorts of ictus in Plautus, with the same in Terence. If we were to proceed in this way with the two books of Virgil just mentioned, we should find the ratio of conflicts to harmonies to be as follows:

Book x., 6th foot, 91 : 1; 5th foot 151 : 1.

" ix., " 273 : 1; " ∞ : 1;

a result which is absurd, for it is evident that in both books the conflicts in question were to a great, and about the same, extent avoided, while this result gives us for the harmonies in the sixth foot,

$$x : ix :: 1 : 3,$$

and for the fifth foot,

$$x : ix :: 1 : \infty;$$

whereas the true relation may be found by omitting the very rare conflicts in both and reducing, and we have, for the sixth foot,

$$x. : ix. :: 135 : 136,$$

and for the fifth foot,

$$x. : ix. :: 272 : 273,$$

and this is about the true relation subsisting between the two books. Now let us compare Plautus and Terence in the same way. Out of ninety-six instances in Plautus, we have

$$\text{—} \text{—} \text{—} \text{—}, 88 + \text{—} \text{—} \text{—} \text{—}, 8 = 96, \text{ for } \frac{88}{8} = 11, \text{ the ratio in Plautus.}$$

In Terence,

$$\text{—} \text{—} \text{—} \text{—}, 84 + \text{—} \text{—} \text{—} \text{—}, 12 = 96, \text{ for } \frac{84}{12} = 7, \text{ the ratio in Terence.}$$

Now, as the small numbers 8 and 12 do not differ so much from each other but that accident could modify them materially, we have as much right to assume 88 : 84, or 22 : 21 as the ratio between Plautus and Terence, as to assume 11 : 7; and, as I have shown from Virgil, this is the much more rational method of the two. Hence, it is quite possible that the whole difference resulted from accident; for it would have been surprising to me if they had not differed to that extent from each other.

d. Words of the form — — — — receive the ictus somewhat more frequently on the antepenult than on the syllable preceding it, which is easy to understand; for, *first*, if the ultima is short and the ictus falls before the accent, another must fall immediately after it, — — — —, which is not allowed. In the *Birds* there are eight examples of this ictus; in *Casina* and *Phormio* not one. This cannot be attributed to any peculiarity of the Latin scheme, nor could chance have caused it; for the same relations exist between *all* the Greek and Latin plays I have examined; and if any one will write a few verses admitting resolved arsis, he will find himself employing this ictus by no means rarely. Accordingly, the few verses which I have appended, as they were originally written, contained two instances of it:

tum ego vósque, nosque *dēcidimus* omnes humi,
dum insultat in nos prōditib' trucissimā.

But as soon as I wrote these verses, I suspected that there was something wrong, because they sounded unnatural and unusual; and it was this which first led me to examine the subject of this particular sort of ictus. These two verses, and these alone, I afterwards changed on this account; but the changed forms exhibit as many conflicts as these (but they are admissible conflicts), so that the whole number of conflicts in the verses is not at all modified by the correction. Even in Seneca, where words containing long syllables often show conflict in the odd places, you will never find this ictus ($\text{—} \cup \cup \cup$), but always either $\text{—} \cup \cup \text{—}$ or $\text{—} \cup \cup \text{—}$.

Secondly; if the ultima is long, we find either $\text{—} \cup \cup \text{—}$, or, what is more frequent, $\text{—} \cup \cup \text{—}$. But here no influence is to be attributed to accent. The form $\text{—} \cup \cup \text{—}$ is rarer for the reason that it is not so well adapted to the scheme of the verse; for it is not possible for the anapaest in $\text{—} \cup \cup \text{—}$ to occupy the first place, and in the second, where the strongest ictus falls, it is unpleasant on an ultima unless the foot is pure; while if it occupies the third place, it prevents caesura and causes incision in the middle of the verse. There only remain, then, the fourth and fifth places, the former of which gives the same difficulty in a less degree than the second place gives, while in the fifth place nearly all the examples occur. So that it is manifest that the ictuses $\text{—} \cup \cup \text{—}$ are not to be expected as often as $\text{—} \cup \cup \text{—}$.

Thirdly; the first syllables of these words, being long, have sufficient emphasis without ictus, and if the ictus falls on it, it does not extend itself to the accented syllable as in $\text{—} \cup \cup \cup$. Frequently, however, we have elision, whence we obtain $\text{—} \cup \cup (\text{—})$, — , which is suited to several places in the verse.

Fourthly; in the Latin dramatists, the anapaest is less frequently employed anyhow than the dactyl, even if we omit the examples contained in the words under discussion. In *Andria* these relations exist:

$\text{—} \cup \cup : \cup \cup \text{—} = 3 : 2$,

but in the *Birds*,

$\text{—} \cup \cup : \cup \cup \text{—} = 1 : 2$.

This circumstance alone would make — ◡ ◡ | ≡, ◡ | more frequent than ◡ | ◡ ◡ ◡ |.

Among all the early dramatic writers, I find nearly the same usage with respect to ictus on words of the forms — ◡ ◡ ≡ and ◡ ◡ ◡ ≡. In Ribbeck's Edition, I find the following relations :

Livius Andronicus,	◡ ◡ ◡ - - -	1,	◡ ◡ ◡ - - -	0
Statius Caecilius,	" - - -	13,	" - - -	2
Sextus Turpilius,	" - - -	7,	" - - -	1
Lucius Afranius,	" - - -	14,	" - - -	0
Lucius Pomponius,	" - - -	7,	" - - -	1
Novius,	" - - -	7,	" - - -	0
Laberius,	" - - -	12,	" - - -	1
Syrus,	" - - -	3,	" - - -	0

Now, if we add to this list :

Plautus,	◡ ◡ ◡ - - -	11 × n,	◡ ◡ ◡ - - -	1 × n;
Terence,	◡ ◡ ◡ - - -	7 × n,	◡ ◡ ◡ - - -	1 × n,

who will deny that the difference between the two may be the result of accident ?

In the so-called "Syri Sententiae," which are of late origin, I find ◡ ◡ ◡ not much more frequent than ◡ ◡ ◡.

§ 6. Briefly to sum up the whole matter, we announce this law : Ictus must not fall on the syllable which immediately follows the accent, except when the arsis is long and the accented syllable short, which happens only in iambic words, as ◡ ◡ ; and the law is especially stringent when another ictus would also immediately precede the accent, as ◡ — ◡ or ◡ — ◡ ; but the law does not apply to the odd places except when short syllables are concerned.

Many minutiae cannot be included in a brief summary.

I do not assert that the poets ever took conscious notice of the accent ; but I have no doubt that if their attention had been called to it by the question why they avoided certain combinations, they would have seen that it was unpleasant relations of ictus to accent that they were avoiding.

These laws were not of an absolute and inviolable nature, but were rather aesthetic ; consequently occasional violations of most of them are sometimes found in verses which cannot be changed, or, at least, which there is no MS. authority for changing.

Here the discussion must be closed. If nothing else worth while has been developed by it, one fact has certainly been demonstrated: that in metrical questions the indolent method of regarding a *post hoc* always as a *propter hoc* cannot be relied upon.

MARCUS ANTONIUS (loquitur).

Lacrimas si habetis, nunc parate fundere.
Vidistis hanc togam, optimi: tempus bene
Memini quo eâ primum amicuit se Julius:
Æstate erat sub vesperum in prætorio
Illo die quo Nervios devicerat.
Spectate: Cassi sica perforavit hic:
Videte quantam fecerit Casca invidus
Scissuram: amatus Brutus hic ictum dedit,
Et cum nefandum ferrum is inde evelleret,
En sanguis ipse Cæsaris secutus est,
Tamquam foras exiret ut pernosceret
Num tam impulisset Brutus ingrato modo.
Nam Brutum amores scitis omnes Cæsaris
Fuisse: quantum amarit æstima, O deus!
Hæc plaga crudelissima omnium fuit;
Nam cum videret Cæsar ipsum pungere,
Ingratus animus proditorum fortior
Pervicit armis: tum cor amplum rumpitur,
Togaque voltu procidit tecto ad pedes
Atro cruore defluentis interim
Signi usque Pompei ipse Cæsar maximus.
Et quantus ille casus, O cives, fuit!
Ego vosque et omnes cecidimus nos tunc humi
Dum insultat in nos sæva proditio insolens.
Hei fletis! atque cerno iam sentire vos
Vim misericordiæ; hæc benigno ex fonte sunt
Guttæ: pii, quid! Cæsaris nostri togam
Scissam videntes fletis? Huc spectate nunc:
Hic ipse—miserum!—lacer ab infidissimis!

‘Ο Κάτων τάδ’ ἑαυτῷ διελέχθη, Φαίδωνα Πλάτωνος ἀναγνοῖς·

Ἀληθές εἶναι τοῦτ’ ἀναγκαῖον· Πλάτων,
καλῶς λέγεις· πόθεν γάρ, εἴγ’ ἄλλως ἔχει,
ἢ τ’ ἐλπίς ἦδε καὶ ποθῶν ὁδ’ ἴμερος
τῆς ἀθανασίας; καὶ πόθεν δέος τόδε
κρυπτόν, τὸδ’ ἐντὸς δεῖμα γίνεσθαι ποτε
τὸ μηδέν; ἐκπληγείσα δ’ ἡ ψυχὴ φθορᾷ
τί ἐῖ παρ’ αὐτὴν χάζεται φοβούμενη;
τὸ θεῖον ἐστὶν ἐν φρεσὶν κινούμενον,
αὐτός τε μέλλον οὐρανός τι δεικνύων
καὶ τοῖσιν ἀνθρώποισιν ἐκφαίνων σαφῶς
τὴν αἰδιότητ’ — ὦ νόημα προσφιλές
καὶ δεινόν, ὡς μὲν πολλὰ καὶ παντοῖα χρὴ
ἡμᾶς ἀπείρατ’ ἐκπερᾶν, οἷας δὲ χρὴ
μεταλλαγὰς καὶ καὶν’ ὁράματ’ εἰσορᾶν.
πλατεῖα κεῖται κᾶμετρος προῦμοῦ θέα,
ἀλλ’ ἔστ’ ἐπ’ αὐτῇ καὶ σκίασμα καὶ νέφος,
σκότος τε. τοῦδε γοῦν ἐγὼ μὲν ἔξομαι·
ὑπερθεὶν ἡμῶν εἶγε που ὅστι κύριος
(καὶ μὴν ἐν ἔργοις πᾶσιν ἡ φύσις λίγα
ὥς ἔστιν αὐδᾷ) τοῖς καλοῖς οὐκ ἔσθ’ ὅπως
οὐχ ἡδεται μὲν, οἷς δ’ ἐκεῖνος ἡδεται
εὐδαιμονεῖν χρὴ ταῦτά γ’, ἀλλὰ ποῦ; πότε;
οὗτος γὰρ οὖν ὁ κόσμος ἐστὶ Καίσαρος.
ἦδη δὲ κάμνω συμβαλὼν· νῦν παυστέον.
καὶ κάρτα δίσσ’ ὅπλ’ ὧδ’ ἔχω· μύρος τ’ ἐμὸς
ζωὴ τε, πῆμα καὶ λύσις, πάρεισί μοι.
ὁ μὲν μ’ ἄγει πρὸς τέρμα, καὶ ταῦτ’ ἐν ῥυχαεῖ,
ἢ δ’ ὡς θανοῦμαί γ’ οὐποτ’ ἐμφανῶς λέγει.
ψυχὴ δ’ ἔχουσα τὴν ὑπαρξιν ἀσφαλῆ
ξίφει γελᾷ σπασθέντι καὶ θαρσεῖ δ’ ἀκμήν.
τὰ μὲν γὰρ ἄστρα φθίσεται, τὸν θ’ ἥλιον
αὐτὸν σκοτώσει δι’ τὸ γῆρας, εἰς τ’ ἔτη
δύναι πέπρωται τὴν φύσιν· σὺ δ’ ἄφθιτον
ἦβην φυλάξεις, ἀβλαβής τ’ ἐν τῇ μάχῃ
ἦν τοί ποτε στοιχεῖα συμβαλεῖν χρεῶν,
κτύπῳ τε κόσμων κοῦσίας ναυαγίῃ.

VIII.—*The Algonkin Verb.*

By J. HAMMOND TRUMBULL.

W. von Humboldt maintained that genuine grammatical forms are found only in the “complete” inflectional languages, namely, Indo-European and Semitic, and that neither agglutinating nor incorporating languages can “really attain to the expression of a true conception of one such form.”* His classification of the “less complete” languages is founded chiefly on differences in their treatment of the *Verb*. In some—the Malay-Polynesian family, for example,—the peculiar function of the verb has *no* characteristic expression; in others, this function is indicated by pronominal affixes. In latter class Humboldt placed “the Mexican, the *Delaware* and other American tongues.” His illustrations of the mode of structure of these languages are mostly taken from the Mexican, the Maya of Yucatan, and the Yarura of New Grenada. For his knowledge of the Algonkin languages, he relied mainly on Duponceau’s Correspondence with Heckewelder and translation of Zeisberger’s Delaware Grammar, and unfortunately he adopted Duponceau’s mistaken notion of American polysynthesis—as effected by “putting together portions of different words, so as to awaken at the same time in the mind of the hearer the various ideas which they separately express.”†

Professor H. Steinthal, in his psychological classification, regards the American languages as “formless.” Selecting as types the Mexican and the Greenlandish, he decides that the former “has, in its method of word-making, formed nouns, but *no true verbs*,” and that the so-called Mexican verb is a noun with a predicate-prefix: e. g. *ni-tlàtlakoāni* ‘I, sinner,’ for ‘I sin;’ *ni-kwalli* ‘I, good,’ for ‘I am good.’‡ In the Greenlandish, Professor Steinthal finds that “the sen-

* *Ueber die Kawi-Sprache*, I. (Einleitung,) cxlvi.

† Preface to Zeisberger’s *Grammar*, p. 20; cited in *Kawi-Sprache*, I. (Einleitung,) ccxxxii, ff.

‡ *Charakteristik d. hauptl. Typen d. Sprachbaues*, 216, 218.

tence is not founded on subject and predicate," but centres in the object; "the verb is without a copula, and the noun without subjective character;" and, in the process of incorporation, "the objective relation is regarded rather as a peculiar sort of *possession*, and the *possessive* which is also a *substantival* relation dominates the phrase." "The Object," he says, generally, "presents itself so forcibly to the consciousness of an American, that he apprehends the existence of the Subject only in its relations to the Object and hence overlooks the earlier and more important relation of the Subject to the Activity." *

Professor Fr. Müller, in his memoir on the grammatical structure of the Algonkin languages (1867) and more recently in his *Allgemeine Ethnographie* (1873) concedes true verb-forms to the Mexican and Dakota languages, but denies them to the Algonkin and Iroquois. "The Algonkin languages," he says, "do not distinguish the noun and verb from each other. From the etymological point of view they know only a *noun*, which, when equipped with possessive-suffixes, corresponds to the expression of our verb." He regards "the want of distinction between *subjective* and *possessive* pronominal-elements" as an essential characteristic of these and many other American tongues. "They recognize only the *dependent* relation, and hence are in a position to form nouns but no *verbs*." The so-called verb is a "nomen actionis," with or without possessive pronominal affixes: for example, Cree *ni-pimootan* ('I walk') resolves itself into 'my walking'; Algonkin *ni-sakih-a* ('I love him') is 'my love to him'; *ni-sakih-ik* ('he loves me') is 'my being-loved by him'; and *ni-sakih-igo* ('I am loved') is 'my being-loved by somebody.' †

M. Abel Hovelacque, follows Fr. Müller, in making the verb, in Algonkin and many other American languages, "nothing but a noun accompanied by suffixes denoting possession." ‡

* *Ibid.*, 226.

† *Der grammat. Bau der Algonkin Sprachen*, 136, 139: *Allgem. Ethnographie*, 275, 276. [Compare his *Grundriss d. Sprachwissenschaft*, I. (Wien, 1876,) 124.]

‡ *La Linguistique* (Paris, 1876), p. 117.

A very different conclusion was arrived at by Bishop Baraga, after twenty years' study of the Chippeway, an Algonkin dialect: "This language," he wrote, "is a language of *verbs*. . . . *All* depends on the *verb*. . . . Where other languages will employ a substantive, the Otchipwé uses a verb."* So, Father Lacombe, who has lived as many years among the Crees and has published an excellent grammar of their language, declares that it is "nothing but a language of verbs."† And Mr. Joseph Howse, in his Cree Grammar (p. 15) asserts that "the Indian verb, as respects its *nature* or *essence*, may, without hesitation, be said to be strictly analogous to the part of speech in European languages, bearing the same name; viz., as *predicating* being, or manner of being or acting." In fact, no one who has learned to speak and write an Algonkin dialect and who has studied its grammatical structure without previous knowledge of its "inner form," as seen by the light of ethno-psychology, has suspected that the Verb — on which "all depends," and to which all other parts of speech are resolvable, — is really *no* verb, but only a disguised noun; that it is not predicative; that its pronominal affixes are merely possessive, not subjective; and that "the idea of time and mode is altogether absent from it."

I propose, in this paper, to examine the grounds on which the genuineness of verb-forms in Algonkin languages has been denied. Before entering on this examination it will be necessary to notice some peculiarities of the grammatical structure of these languages.

1. The distinction between *animate* and *inanimate* objects — or, more exactly, between objects *regarded as* belonging to the one or the other of these two classes — is never lost sight of by the Algonkin speaker. It is constantly recognized, not only in the formation of the plural of nouns but in every expression of relation. It modifies every conjugation-form of the verb, according as the subject or the object is animate or inanimate.

2. Formal distinction of *number* seems to have been earlier established for animate than for inanimate nouns. The char-

* *Otchipwé Grammar*, pp. 15, 28.

† *Grammaire Crise*, p. 53.

acteristic of the animate plural (*k*, or its sonant, *g*) is the same in all Algonkin dialects; but there is much diversity in the forms of the inanimate plural; in the Cree, it adds *a* to the singular; in Chippeway, *n* preceded by a vowel; in Abnaki, *r* or *l*; in Illinois, *ri*; in Massachusetts and Quinipiac (or Quiripi), *sh*.

3. Every dialect has a law of euphony which regulates the vocal connection of the sign of the plural with a noun in the singular. In the Chippeway and (Nipissing) Algonkin, if the singular ends in a vowel, *k* is added to form the animate, *n* for the inanimate plural; if the singular ends in *k*, the animate plural adds *ok*, the inanimate *on*; if the singular ends in any consonant except *k*, the animate plural adds *Δk*, the inanimate, *Δn*.*

ANIMATE NOUNS.

Nipiss. Alg.,	<i>makwa</i> , a bear,	pl. <i>makwak</i> ,
Chip.,	<i>makwa</i> ,	" <i>makwag</i> ,
Cree (Western),	<i>maskwa</i> ,	" <i>maskwak</i> ,
" (Eastern),	<i>müskwa</i> ,	" <i>müskwük</i> ,
Mass.,	<i>mosq</i> , <i>mashq</i> ,	" <i>mosquag</i> ,
Muhhek.,	<i>m'quoh</i> ,	" <i>m'quohk</i> .
Nip.-Alg.,	<i>akik</i> ,† a kettle,	pl. <i>akikok</i> ,
Chip.,	<i>akik</i> ,	" <i>akikog</i> ,
W. Cree,	<i>askik</i> ,	" <i>askikwok</i> ,
Mass.,	<i>ahkühk</i> ,	" <i>ahkühwog</i> ,
Blackfoot,	<i>tski</i> , <i>isk</i> ,	" <i>isk-iks</i> .

INANIMATE NOUNS.

Cree,	<i>sípi</i> , a river,	pl. <i>sípia</i> ,
Chip.,	<i>síbí</i> ,	" <i>sibiwan</i> ,
Abnaki,	<i>sipu</i> ,	" <i>sipuar</i> ,
Illinois,	<i>sipíwi</i> ,	" <i>sipíwari</i> ,
Mass.,	<i>sipu</i> , <i>síp</i> ,	" <i>sipüash</i> .

4. Certain relations which nouns may sustain to each other, or to the principal action of the sentence, are expressed by changes of termination, that is, by *inflection*. Other relations are expressed by prefixes and suffixes which have not, in composition, entirely lost traces of original independence.

*Cuoq, *Études philologiques*, 36.

† This is one of the nouns which is classed as *animate*, in all Algonkin dialects.

The modifications of the noun correspond so nearly with those of the governing verb, that case-endings are not easily distinguished from conjugation-forms. M. Cuoq says: "Algonkin nouns are *conjugated*, not *declined*;"* and he includes the relations expressed by cases under the general name of "*accidents* which may modify nouns." Of these accidents he enumerates (in the Nipissing-Algonkin dialect) twelve, only four of which can properly be regarded as *cases*: namely, the locative, the possessive, the obviative ("obviatif"), and the super-obviatif ("surobviatif"). In these, the vocal connection of the case-ending with the noun is regulated by the same euphonic law which prescribes the vowel to be used in forming plurals.

The *locative* case indicates the place or object *to*, *at*, *in*, or *on* which the action of the verb is directed. Its formal characteristic varies in different dialects. In the Chippeway, Nipissing, Ottawa, etc., it is *ng*; in Delaware, *nk*; in Cree, *k* or *g*; in Massachusetts, *t*.

The *possessive* case, by which property or possession is expressed more emphatically than by a pronominal prefix, is formed by adding *m*, *om*, or *im*: e. g. (Chip.) *nind aki* 'my land,' 'my country'; *nind akim* 'my own (piece of) land'; *kid ikwe* 'your woman,' *kid ikwem* 'your own woman,' 'your wife'; *nind akikom* 'my own kettle'; etc.†

The *obviative* and *super-obviative* are forms of what Mr. Howse (*Cree Grammar*, 125) calls the "possessive or accessory case," which "serves" to distinguish the *accessory* or dependent from the *principal* or leading third person." Algonkin grammar recognizes *three* third persons, singular and plural. When two nouns (or a noun and a pronoun) in the third person are introduced in the same sentence, one as the subject and the other as the object of a verb, the latter takes the *obviative* — or second third-personal — form. When *three*

**Etudes Philologiques*, 38.

† "The *possessive rank* of nouns, is when the *person* doth challenge an interest in the *thing*. . . . And it is made by adding the syllable *eum*, or *oom*, or *um*, according to euphonic, unto the affixed noun. For example, *num-Manitloom*, my God; *num-moskeht eum*" [my meadow] — Eliot's *Grammar*, 12.

nouns animate or pronouns in the third person are used, the third of which stands in a dependent relation to the second, the subject of the verb has the simple form (first third-person), and of the other two, one receives the obviative (second third-personal) and the other the super-obviative (third third-personal) inflection.

The characteristic of the obviative in the Chippeway dialect is *n* (with a connecting vowel); in Cree, *a*; in Delaware, *l*; in Massachusetts (in Eliot's notation) *h* after a vowel. *Inanimate* nouns do not change their terminations in the obviative. The characteristic termination of the super-obviative in Chippeway is *ni* (with connecting vowel); in Western Cree, *iyiwa* for animate nouns (sing. and pl.), *iyiw* (sing.) and *iyiwa* (pl.) for inanimate.

When the object of a verb is in the obviative, *the verb itself receives the obviative inflection.*

How important a place these "accessory cases" fill, in the construction of Algonkin sentences, will be seen by a few examples. The first are in the (Nipissing) Algonkin dialect, the others from the Chippeway (of Baraga):

1st 3d person, *eebik asapike*, the spider makes a web.

" " *ni-nisa eebik*, I kill a spider.

Obviative, *Jan o-nisan eebikon*, John kills a spider.

1st 3d person, *Ki takona wagosh*, You catch a fox.

Obviative, *Simon takonan wagoshan*, Simon catches a fox.

Super-obviative, *Bazin o-pakitean Pienan o-kwisisini*, Basil he strikes Pierre's son.

Here 'Basil' is in the *first* third-person, 'Pierre' (Pien) in the *second*, and 'his son' in the *third*. If the proper names are omitted, the distinction of these persons is still preserved by the verb form and the case-endings: *o-pakitean o-kwisisini* 'he (1st 3d p.) struck-him (2d 3d) his-son (3d 3d.)'

*Cuoq, *Etudes philologiques*, 43. In the Yakama (Sahaptin) language, "if the governing substantive is itself governed by a verb, the substantive which it governs takes the compound termination, and is put in the same case as that which governs it. The compound terminal sign is only the union of the dative, or the accusative, with the genitive." Example: *miawar-nmi-ow nit-iow nes winasha* 'I am going to the chief's house,' in which *miawar* 'chief' takes the affix of the genitive (*nmi*) and dative (*iow*), and *nit* 'house,' that of the dative only.—Pandosy's *Yakama Grammar*, 29.

Chippeway :

1st 3d pers. *nin sagia noss*, I love my father.

2d 3d " *kwiwisens o-widokawān ossān*, the boy helps his father.

3d 3d " *Joseph o-gi-odapinān abinōdjilān oginī gaie*, Joseph took the young-child (2d 3d p.) and his-mother (3d 3d p.)

3d 3d pers. *Paul o-gi-nissān ossaieian, wiwinī gaie*, Paul killed his-brother, and his [brother's] wife.

2d 3d pers. *Paul o-gi-nissān ossaieian, wiwān gaie*, Paul killed his brother, his [Paul's] wife also.

Eliot observed, but did not clearly understand, the use of the "obviative" in the Massachusetts dialect. "There seemeth," he says,* "to be one *cadency* or *case* of the first declension, [i. e.] of the form *animate*, which endeth in *oh*, *uh*, or *ah*; viz: when an animate noun followeth a verb transitive, whose *object* that he acteth upon is *without himself*:" e. g. *neemun namohs* "take up the fish" (Matt. 17. 27), but *quagwashwéhtauāu mishe namohsoh* "he prepared a great fish" (Jonah, 1. 17), where the noun receives the case-ending of the second third person.

5. All Algonkin nouns whose derivation can be traced are formed either directly *from verbs* or on distinctively *predicative* roots,† and retain much of the nature as well as of the forms of verbs. They express relations of time and manner by affixes and internal change of vowels. Many, including all names of action, have distinct *active* and *passive* forms. They are qualified by adverbs, not by adjectives. The author of *Études Philologiques* scarcely exceeds the fact, in the assertion that "les noms algonquins ne se déclinent, ils se conjuguent." But though the noun so commonly assumes some of the forms it does not usurp the office of the verb. It is always a *verbal*, but cannot — without change of termina-

* *Indian Grammar Begun* (1666), p. 8. Eliot has but one form for the obviative, in the singular and the plural. He wrote, *togkomau ketassootoh* "he smote the king" (2 K. xv. 5), and *wehkomaui nishwe ketassootoh* "he called three kings" (2 K. iii. 10). So *ayim anogqsoh* "he made the stars" (Grammar, p. 8)—*anogqs* (pl. *anogqsog*) being classed with animate nouns.

† This holds true even with respect to onomatopoeic names. Mass *kōkontu* 'a crow' has the predicative termination, 'he *kōkonts*'; *quequkum* 'a duck' (an abbreviation, as the plural form shows, of *quequkummau*) has the characteristic verbs expressing *action of the mouth*: 'he *suys quequek*.'

tion, to express the predicative function—become a *verb*. Examples of several classes of nouns, showing their relations to and modes of derivation from their respective verbs, will be given further on.

6. Analysis of the structure of Algonkin speech exhibits, as its final result, elements exactly corresponding to the *roots* of Indo-European languages; elements which must have “existed prior to the whole development of the means of grammatical distinction, before the growth of inflection, before the separation of the parts of speech,” and which “indicated each some conception in entire indefiniteness as concerns its relations, neither viewed as the concrete name of an object, nor as attribute only, nor as predicate; but as equally ready to turn to the purpose of any of the three.”* The number of pronominal or demonstrative roots seems to be, relatively, greater than in Indo-European speech. Nearly all the predicative, as well as the demonstrative, are resolvable to monosyllables. Modification of the meaning of a root by the addition of a consonant—with or without a connecting vowel—is a prominent feature of this group of languages. The consonants so employed have, each, a determinable and constant value, independent of the principal roots with which they are associated, and serve as “characteristics” of various forms of conjugation, “indicating the *manner* of being, doing, or acting.”† They are properly to be regarded as roots or representatives of *roots*, and not (as Mr. Duponceau and others believed them to be) as mnemonic symbols of *words* which had received grammatical forms.

These uniliteral elements are used as prefixes as well as affixes. Volney‡ observed in the Miami (an Algonkin) dialect that, “generally, *m* begins all words which denote what is *bad* or *ugly*.” Duponceau adopted this statement and drew from it an inference which helped give W. von Humboldt a wrong notion of Algonkin word-making and of American polysynthesis generally: “The mere initial *m* of *machit* ‘bad’ or

* Whitney, on Indo-European roots, in *Life and Growth of Language*, p. 201.

† Howse, *Cree Grammar*, 37 ff.

‡ *Tableau des États-Unis*, ii. 527.

medhick 'evil' gives," says that eminent scholar,* "a bad and depreciatory meaning to a word." Whatever "bad meaning" a prefixed *m* imparts comes from the root of which *m* is the representative and not from the derivatives *machit*, *medhick*, or *matta* 'not,' or even from the particle *mo* of which *m* is the base.

The most simple forms under which Algonkin roots assume grammatical relations are — the pronouns and a few demonstrative particles excepted — *verb* forms. "The making of a verb is nothing more than the establishment of certain combinations of elements in an exclusively predicative use, the supplying a copula in connection with them and not with others;"† and this is accomplished in Algonkin as in Indo-European speech, by associating a pronominal with a verbal element. The predicative function receive as distinctive expression and the verb-form is as really attained in Chippeway *AB i* (Cree *AP-iu*, Abnaki and Massach. *AP-u*), 'he stay-s, abid-es, remain-s,' as in Gr. *MENε* or Lat. *MANet*. It is true that we find in Algonkin verbs — even among those which convey elementary ideas and which, presumably, are of very early formation — considerable diversity in the formal expression of the predicate relation. This diversity is owing partly to the distinction between animate and inanimate subjects, and partly to the want of a *complete* independent verb-substantive. Hence come a great number of conjugation-forms unknown to Indo-European grammar. Baraga reckons *nine* conjugations, for the Chippeway verb, distinguished by the termination of the third person singular of the indicative present; Zeisberger makes eight for Delaware verbs; Howse and Lacombe distinguish seven forms of conjugation of Cree intransitives and the latter gives eight to verbs transitive-animate.

For the purposes of this paper, we need not look beyond the most simple forms — those of verbs neuter (subjective), active-intransitive, and transitive-absolute (without an expressed object).

* *Die Kawi-Sprache*, i. (Einleitung) cccxxxiii.

† W. D. Whitney.

In the conjugation of intransitive (primary) verbs, the pronominal element, which indicates the predicative function, is *suffixed* to the verbal root, in the *third* person, in direct assertion, and is *prefixed*, in the *first* and *second* persons. In conditional or indefinite assertion, in command and prohibition, the verbal root is without pronominal prefix, and the grammatical persons, singular and plural, are distinguished only by the *terminations*, which retain no trace — except, perhaps, in the first person singular of the subjunctive — of original independence. “The conjugational form of the Cree intransitive verb has” — as Mr. Howse expresses it* — “in the indicative mood [the third person excepted], the *prefixed* nominative of the French; and in the subjunctive, the *inflected* personal termination of the Italian and Latin.” The 1st and 2d persons of the indicative are distinguished only by their pronominal prefixes, both having the same termination, which is, usually — in the Cree language, always — *n* (with a connecting vowel: but in some dialects — particularly, those of the Chippeway group — many verbs whose 3d person ends in a vowel, have lost the characteristic termination of the first and second persons; e. g.

† Cree, <i>ap-iü</i> ‘he stays,’	1st p. <i>n’t ap-in</i> ,	2d p. <i>k’tap-in</i>
Chip. <i>ab-i</i>	<i>nind† ab</i>	<i>kid ab</i>
Mass. <i>ap-u</i>	<i>n’t ap</i>	<i>k’t ap,—</i>

though Eliot occasionally uses the forms *n’t ap-in*, *k’t ap-in* (*nut appin*, *kut appin*, in his spelling). So, Cree *posiü* ‘he gets into a canoe, embarks,’ (1st p.) *ni posin*; Chip. *bos-i*, *nin bos*; Abn. *poos-u*, *ni poos-i*: Cree, *nip-iü* ‘he dies,’ (1st p.) *ni nip-in*; Alg. *nip-o*, *ni nip*; Chip. *nib-o*, *nin nib*; Mass. *nüpp-u*, *n’ nüp* [*nunnup*, El.]; with which compare, Cree

* Cree Grammar, p. 77.

† The authorities on which I have chiefly relied are, for the Chippeway (Chip.), Baraga’s Grammar and Dictionary, and for the closely related Nipissing Algonkin (Alg.) Cuoq’s *Etudes Philologiques*, etc.; for the Cree, Lacombe’s and Howse’s Grammars and Lacombe’s *Dictionnaire de la langue Crise*; for the Abnaki (Abn.), Rasles’ Dictionary (of the Kennibé or Caniba dialect); and for the Massachusetts (Mass.), Eliot’s Grammar and his version of the Bible.

‡ “To the pronouns: *nin* ‘I’ and *ki* ‘thou,’ ‘we,’ a euphonical *d* is attached when the following verb commences with a vowel.” — Baraga’s Grammar, 43.

nip-aii 'he sleeps,' (1st p.) *ni nip-ān*; Alg. *nip-e*, *ni nip-a*; Chip. *nib-ā*, *ni nibā*.

Imperative. There is much diversity, in Algonkin dialects, in forms of the imperative. The 2d person plural, in all dialects, ends in *k* (or *g*). For the 3d person, singular and plural, the Chippeways and Crees use the 3d person of the indicative present, with a prefixed particle. The Massachusetts has special forms for the 2d and 3d persons, in both numbers: Sing. 2d p. *apsh*. 'sit thou'; 3d p. *apitch* 'let him sit'; pl. 2d p. *apik* or *apigk*; 3d p. *apihettitch*.

The *Subjunctive* "or rather the *Supposing* or *Suppositive* mode," says Eliot (Grammar, 19, 26), "when the action is only supposed to be,—usually flats the first vocal and lays by the *affix*," i. e. the pronominal prefix. Mr. Howse calls this "flattened vowel of the subjunctive, the sign of an *indefinite tense* or time,"—"signifying that the action combined with the attribute is generalized, or rendered indefinite in respect to *time*, and hence, secondly, implying sometimes *custom*, or *habit*, in the subject of the verb" (Cree Grammar, pp. 71, 73). Baraga gives a full account of this "vowel change," which, he says, "embarrasses much the beginning learner of the language" (Otc. Gram. 128-141). The change is not accurately described as a *flattening*. It is in fact a *strengthening* of the first vowel of the root, or stem,—or of the tense-particle prefixed,—by changing *

a long to *aié*,
o long to *uá* (*wa*),
i long to *á*,

e long to *aié*,
o short to *ué* (*we*),
ā or *ĩ*, short, to *é*.

The author of *Etudes Philologiques* (p. 49) terms the modes in which this change takes place, the "eventual," the "simultaneous," the participial, and the gerundive. Baraga says that it takes place (1) in all *participles*; (2) in sentences expressing *periodical* actions, events, or states of being, or (3)

* For the Chippeway and Nipissing-Algonkin (Baraga, Otc. Gr., 129; Cuq, Etudes, 49). I substitute *u* for Baraga's *w* and Cuq's *ø* (Eliot's *ω*). Lacombe, for the Cree, has a somewhat different notation (Gram. Crise, 155): *a* long, to *iya* and *eya*; *e* long, to *iye*; *o*, after a consonant, to *oyo*; *o*, beginning a word, to *we*; *i* long to *iye*; *a* short to *e* long; *i* short, to *e*.

actions *just past*; (4) after certain interrogations, meaning, how? what? when? where? etc.; (5) in sentences expressing *comparison*; and (6) in sentences that express *quality* after adverbs meaning 'all,' 'all that,' 'whatever,' 'whosoever'; (7) in some tenses of the subjunctive mood in the *dubitative* conjugation; (8) after *mi* 'so'; and (9) in sentences which in English would contain a *relative pronoun* before the verb, 'he who' or 'that which,' etc. For our present purpose, Mr. Howse's more comprehensive statement of the effect of this vowel-change is sufficiently accurate: it "signifies that the *action* combined with the attribute is *generalized*, or rendered *indefinite* in respect of *time*, and hence implying sometimes *custom* or *habit* in the *subject*; whence it also often becomes the Indian equivalent of *English nouns ending in er and implying an actor*."

Edwards, in his "Observations on the Language of the Muhhekaneew Indians," and Zeisberger, in his Delaware Grammar, name this form of the verb, a *participle*. "Though the Mohegans," says Edwards, "have no proper adjectives, they have participles to all their verbs: as *paumseet*, the man who walks [from *pumissoo*, he walketh]; *waunseet*, the man who is beautiful [from *w'nissoo*, he is beautiful]; *oieet*, the man who lives or dwells in a place [= *āyit*, Eliot, from *ayeu*];" etc. "They have no *relative* corresponding to our *who*, or *which*. Instead of 'the man who walks,' they say, 'the walking man' or 'the walker.'" A more accurate translation is, 'he *when* walking,' i. e. *conceived as walking*, or 'as a walker.'

The "vowel change" is of very frequent occurrence in Algonkin speech. "It occupies us in all our conjugations," says Baraga, who has devoted more than twenty pages to its illustration. Lacombe observes that it presents one of the greatest difficulties of Cree Grammar, but that he who has not learned when and how to use it is a bad Cree scholar. It characterizes every participle, is employed in almost every conditional clause, and commonly in interrogation; it supplies the place of relative pronouns, and serves to form all general names of *actors* or *doers*. No view of the structure

of Algonkin language which leaves out of sight the *changed vowel* can be even approximately complete: and yet this feature seems to have escaped the observation of Humboldt, Steinthal, Fr. Müller, and many other less eminent writers on American languages.

The grammatical persons of the subjunctive or "suppositive" mood are distinguished by *terminations*—and *not* by pronominal prefixes. In the Massachusetts dialect, the first and second persons plural are distinguished only by the accent or quantity of the vowel of the last syllable: e. g. "*naüm-og* (*o* as in *log*), if we see; *naüm-óy* (*o* as in *vogue*), if ye see." (Eliot's Gr., 3.) In the Chippeway, the first and second persons singular are similarly distinguished: "the termination of the first person, *iàn* or *ân*, is pronounced long; whereas that of the second person, *ian* or *an*, is very short." (Baraga, 118.) The 3d person sing. (animate) ends, in all dialects, in *t* or *d*. For example: Chip. *ābi* (Mass. *āpu*) 'he sits,' makes in the suppositive, with vowel-change,

- sing. 1. *ēpi-iàn*, if (or, when) I sit, I who sit, I sitting.
- 2. *ēpi-iān*, if (or, when) thou &c.
- 3. *ēpid*, if (or, when) he &c., and, as a noun, a *sitter*.
- pl. 1. *ēpi-iāng*, if we &c. (*exclusive*).
- 2. *ēpi-iāng*, if we &c. (*inclusive*).
- 3. *ēpi-ieg*, if you &c. Mass. *ēpiög*.
- 3. *ēpidjig*.*

From Chippeway *mawi* 'he weeps' is formed, in the 3d person, suppositive, *mēwid* 'he who (habitually, or *sometimes*) weeps,' 'a weeper'; from *niba* 'he sleeps,' *nēbad* 'one who sleeps'; from *ākosi* 'he is sick,' or 'in pain,' *aiakosid* 'a sick person'; from *wābi* 'he sees,' *waiabid* 'one who habitually sees'; *nishiwē* 'he kills, murders,' *neshiwed* 'a murderer'; &c.

The Copula. Algonkin languages possessing no independent verb-substantive, bare existence cannot be affirmed as a general attribute alike of *persons* and *things*. The formal

* The Chippeway, according to Baraga and Cuq, has, besides the "eventual" or "participle" forms, a simple subjunctive—'if I sit' &c.—*without the vowel change*. The personal terminations of the two modes are the same except in the 3d person plural, in which Baraga gives *-djig* for the "participle," and *-wad* for the subjunctive proper.

expression of predicate relation is modified by the fundamental distinction between *animate* and *inanimate* being. The functions of the verb 'to be' are performed (1) by pronominal elements, in combination with predicative roots, (2) by tense particles, expressing past, present, and future relations in *time*, and (3) by primary verbs which — independently, or in composition with other verbal roots — affirm *modes* of being, animate and inanimate, active and inactive.

(1.) "We have no complete distinct word for the verb-substantive," says Eliot (*Indian Grammar* Begun, p. 15), "as other learned languages and our English tongue have, but it is *under a regular composition*, whereby many words are made Verb Substantive. . . . The first sort of verbs substantive is made by adding any of these terminations to the word: *yeuoo*, *aw*, *ow* [= *iu-u*, *e-u*, *o-u*], with due euphony: and this is so, be the word a noun, or adnoun, or an adverb or the like"; e. g.

<i>wosketomp</i> 'a man' [vir],	<i>wosketomp-o</i> <i>ow</i> 'he is a man.'
<i>wompi</i> 'white,'	<i>wompi-yeuoo</i> 'it is white.'
<i>nux</i> 'yea,'	<i>nux-yeuoo-utch</i> 'let it be yea' (James v, 12).

"The second sort of verb substantives is when the *animate adnoun* is made the third person of the verb, and so formed as a verb: as *wompesu* 'white,' *naw-wompes* 'I am white,' *kaw-wompes* 'thou art white,' *wompesu* 'he is white.'*

Baraga (*Otch. Grammar*, 87, 410,) gives nearly the same rules for forming "substantive verbs" in the Chippeway. If the substantive ends in a vowel, add *w* [= *ow* of Eliot] — if it ends in a consonant or a nasal, add *iw* or *ow* — to form the *first person* sing. of the indicative present: e. g.

<i>inini</i> 'man,'	<i>nind' ininiw</i> 'I am a man.'
<i>ogima</i> 'chief,'	<i>nind' ogimaw</i> 'I am a chief.'
<i>assin</i> 'a stone,'	<i>nind' assiniw</i> 'I am a stone.'

The third person adds *i*: e. g. *ininiwi* 'he is a man,' *ogimawi*, *assinawi*, etc.

In the Cree, "by adding *wiw*, for the third person singular, you have the auxiliary 'to be'" (Lacombe, *Cr. Gr.* 88): e. g.

* What Eliot denominates "the animate adnoun" is really an animate intransitive verb, which corresponds to our predicate adjective with the copula. See page 164, post.

mistik 'wood,' *mistikowiw* 'he is wood'; inanimate, *mistikowan* 'it is wood.' Howse gives (p. 17) *nápáyoo* 'a man' (vir), *napawoo* 'he is a man'; *hóokemow* 'a chief,' *hóokemowoo* 'he is a chief'; *níppee* 'water,' *níppee-wun* 'it is watery, i. e. possesses the nature of water'; etc.

Generally,—an Algonkin noun or adjective becomes predicative by affixing *u* (*o*, *w*,) with or without a connecting vowel. If the subject is inanimate, *n* preceded by a short vowel is added to the affix.

(2.) Eliot found in the Massachusetts language certain tense particles which supply the place of auxiliary verbs and particularly that of the verb 'to be' in expressing relations in time. For time present, he uses the *personal pronouns* in their independent—that is, in their *predicative*—form, for the 1st and 2d persons (*nēn*, *kēn*); and the demonstrative *noh* (inan. *ne*), for the third person.* e. g.

nen *uosketomp*, "I am a man," Matt. 8. 9.

nen, *nen* *n'noh* "I, even I, am he," Deut. 32. 39.

nen, *nen* *Jehovah* "I, even I, am the LORD."

ken *noh* *uosketomp* "thou art that man," 2 Sam. 12. 7.

sun *KEN* *NOH* *woh* *paont*, "art thou he that should come? Matt. 11. 3.

For the other tenses, we have, in the same dialect,—

koh, *kó*, connecting *past* with *present* time; expressing *continued* being or activity, 'was and is.'

mó, preterit absolute, and emphatic; 'was and is not.'

woh, "expressing the notion of a *possibility to be*" (El. Gram. 20); corresponding to the auxiliaries 'may,' 'can,' 'must,' etc.

toh, "properly signifieth *utinam* 'oh, that it were,' and is to be annexed to every person and variation of the optative mood" (El. Gram. 34).

pish 'he or it will'; simple future.

Examples: *noh* *KOH* *MO*, *noh* *KOH*, *kah* *noh* *paont*, "who was, and [who] is and [who] is to come," Rev. 4. 8: *nen* *NUKOH* [= *noh* *koh*], "I am he," Mark 13. 6: *ken* *NUKOH*, *kah* *ken* *NUKOH* *MÔ*, thou "who art, and wast," Rev. 11. 17.

Eliot occasionally (as in this last example) combines *koh* with *mo*—the former expressing continued *being*, the latter *past time*. So (in Gen. 1. 9, 11), *kah* *mónkó* [= *mó* *koh*]

* He makes only incidental references to these particles in his Grammar, but frequently employs them in his version of the Bible and other translations.

n'niĥ "and it was [and *continues to be*] so," literally, 'it (was) that (which) is so.'

Resolving *noh* to *n* + *ω*, *koh* to *k* + *ω*, and *mō* to *m* + *ω*, we have, as expressions of time—or affirmations of *being* in relation to time—present and past, three pronominal elements, *n*, *k*, *m*, associated with the general demonstrative and predicative *ω* (or *u*).

In the same way, the Yaruras—Indians of New Granada, living near the Meta and Casanare rivers (tributaries of the lower Orenoco)—"construct their whole conjugation," says W. von Humboldt* (on the authority of the Jesuit father Forneri), "in the most simple manner, by combining the pronoun with tense particles. . . . These combinations constitute the verb 'to be,' and, annexed to a word, supply its conjugation-forms. The verb 'to be' has no articulate root peculiar to itself and which belongs neither to the pronoun or the tense-particles; and since the *present* tense has no particle of its own, its persons are expressed merely by the persons of the *pronoun* itself, which are distinguished only by abbreviations of the independent pronouns. The three persons of the verb 'to be,' in the singular, are *que*, *mé*, *dí*, literally 'I,' 'thou,' 'he,' merely. For the imperfect, the syllable *ri* is prefixed: *ri-que* 'I was'; and, in combination with a noun, *ui ri-dí* 'water was' (preterit absolute); as a true verb, *ġura-ri-dí* 'he ate.' Thus *que* signifies 'I am,' and this form of the pronoun *distinctively expresses the function of the Verb.*"

The place of the verb 'to be' is similarly supplied in other American languages—by an abbreviated personal pronoun, with affixes for past and future time. Humboldt† notices the Huasteca and, particularly, the Maya, which "possesses a pronoun that, of itself, constitutes the verb 'to be,' and which manifests very remarkable carefulness to express the true function of the verb, by a distinct element appropriated to that purpose." In the Tupi of Brazil, *poéra* and *râma*—expressing past and future time—combine with personal pronouns to form the auxiliary verb:

* *Die Kawi-Sprache*, i. (Einleitung) s. cclxxxi.

† *Ibid.*, cclxxxiii, ff.

xe-poér, I was (have been),
nde-poér, thou wast,
y-poér, he was :

xe-rám, I will (or must) be,
nde-rám, thou wilt be,
y-rám, he will be.*

Such constructions are not peculiar to languages of the new world or to such as Professor Steinthal regards as "formless." "In the Demotic Egyptian, the copula is usually expressed — as it is always in the Coptic — by the demonstrative *pu* (femin. *tu*, plur. *nau* ; Copt. *pe*, *te*, *ne*), when it is not left absolutely unexpressed."† In Coptic, additional expression is given to the verb in the first and second persons, by repetition of the pronoun, in a modified form: e. g. *anok u uro* 'I (am) a king,' or *anok ang u uro* 'I, I, a king,' i. e. I am a king, or *ang u uro anok* 'I a king I:' with which compare Mass. *nen nen n'noh* 'I, I (am) he,' etc., page 160, ante. In all Semitic dialects personal pronouns are employed, as in Coptic, to supply the copula.

The office of the Algonkin tense-particles is less distinctly marked in modern dialects than in the Massachusetts. In the Chippeway group, Mass. *koh* is represented by *ki*, a sign of the past tense; *ka* (or *ga*), the sign of the indicative future; and (Nip. Alg.) *ke*, for the future of the subjunctive: Mass. *toh* becomes *ta* (or *da*), "denoting condition"; Mass. *woh* is Chip. *wi*, "denoting intention, will," etc.; and these particles are treated as portions of the verbs to which they are prefixed — not as independent words — and receive the vowel-change, in the suppositive mood:‡

(3) There are several primary verbs which — independently or in composition with other verbal roots — affirm modes and conditions of being, animate and inanimate, active and inactive. An *animate* subject 'moves,' or 'sits' or 'abides,' or 'possesses,' or 'is in this or that place'; an *inanimate* subject 'is at rest,' or 'is moved,' or 'is placed,' or 'grows,' or 'belongs to,' or 'is such as.' By prefixing a verbal or a demonstrative root to one of these primary verbs, an "adjective verb" is formed, corresponding to our predicate

* Anchieta, *Gramm. d. Brasil, Sprache*, hrsg. Platzmann, 87.

† Steinthal, *Charakteristik*, 240.

‡ Baraga, *Orch. Grammar*, 134; Cuoq, *Etudes philologiques*, 54, 55.

adjective with the copula (ante, p. 159). Two or three verbs, much employed in composition, have, in some dialects, lost their independence and are reduced to mere formatives of adjective verbs.

The verbs most frequently used as substitutes for the verb 'to be' are (in the Massachusetts dialect) the following:

Ap'u (*appu*, El.) 'he stays, abides, remains, *sits*': *toh kut-apin?* "where art thou" (staying), Gen. 3. 9; *na kut-apin* "there thou art," Ps. 139. 8; *noh apit* [*ēpit*] "he that sitteth," Ps. 2. 4, "he that abideth" (ὁ μένων), 2 John, v. 9; *na apsh* "be there," Exod. 24. 12.

Ai-iu (*ayeu*, El.) 'he is *there*, is in this or that place' (Fr. *il y est*); hence, 'he *dwells*': *ayeu kah appu* "he dwelleth and abideth," Job 39. 28: *nut'ai* "I dwell," Ezek. 43. 9: *matta ayeu-w-og* (negative form) "they *were* not" (there), Jer. 31. 15. This verb is so commonly used in place of the verb 'to be' that it has been regarded as a genuine verb-substantive, by several writers on Algonkin grammar. Mr. Howse (Oree Gr. 136) calls it (Cree *iów*, in his spelling; *ayaw*, Lacombe,) "the verb-substantive in its absolute form, 'he, or it, is *being*, or *existent*,' Anglice 'he, or it, *is*.'" Baraga translates it (Chip. *nind aid*, 3d pers. *aia*) by "I am, I exist," and Schoolcraft gave its paradigm* (as "*i-e-au*, to be") in proof that the Chippeway language possesses a true substantive verb. That it affirms *being in place*, and not *being, absolutely*, is shown by its derivatives, in all Algonkin dialects. Its root is a demonstrative (*yeu*, El.; Chip. *aw*), and its verbal noun, Mass. *ayeu-onk* 'placing,' 'being here, or there,' was used, objectively, for 'a place' or 'a dwelling place.' The participial (3d pers. suppositive), with changed vowel, Mass. *ayit*, El. (= *ēit*), Chip. *eiad* (*i-āt*, Schoolcraft), Moh. *oiet*, means (as Edwards translates it) "the man who lives or dwells in a place," hence, generally, 'dweller' or 'inhabitant' of a place or country.

Unni (and *n'ni*, El.) 'it is so,' the predicative form of the general demonstrative, *ne* 'this,' becomes, in the personal conjugation, *wut-inni-in* 'he is *such as*,' or 'of the kind of,'

* Indian Tribes &c., ii. 436-441.

and expresses the relation of an individual to a species or a class. In this sense, Eliot used it in such expressions as *neáne unnantog ut wuttahhut, ne wuttiniin* "as he thinketh in his-heart so is he," i. e. of that kind is he,* and in the much-discussed translation of "I am that I am," Exod. 3. 14: *Nen nuttinniin nen nuttinniin*, 'I myself am of the kind I myself am of the kind of,' i. e. I am like myself and no other, I am such as I am such as,—Ego sum talis qualis ego sum.

Us-u (*ussu, usseu*, El.) 'he moves, acts,' Lat. *agit*, expresses *animate activity*: Abn. *ned-a'sw* "je fais, j'agis" (Rasles); Cree ✓ *osi, oshi*, "faire, créer" (Lacombe); *áche-oo, áje-oo* "he moves, has the faculty of moving" (Howse). This verb or its predicative root is the base of a great many animate intransitive and adjective verbs. In composition, the short vowel of the root becomes, in the Massachusetts dialect, *i* (in Eliot's notation, *e*), and in the Cree and Chippeway it is modified by or coalesces with a preceding vowel, giving the terminations (in 3d person sing.) *-isiu, -ísiu, -issu, -ésu*, etc. The consonantal element, *s*, is constant, and may be regarded as the characteristic of verbs which express manner of *animate* being. It affirms *life*, or existence as life. As examples of adjective verbs animate ("animate adnouns"), Eliot gives, in the Massachusetts,

menukk-i (it is) strong,

menuhkesu, he is strong.

womp-i, " white,

wompesu, he is white.

wunn-i, " good, well,

wunnesu, he is good :

and in 1st person, *num menuk-es*, I am strong, *noo-womp-es*, I am white, etc.

In Western Cree (Lacombe): rad. *kin* 'long,' adj. v. *kinosiw* 'he is long, tall' ; *miyo* 'pleasing, good,' *miyosiw* 'he is good' ; *ak* 'painful, unpleasant,' *akosiw* 'he is in pain, sick' ; *maskaw* 'it is strong,' *maskawsiw* 'he is strong': to which correspond in (Howse's) Eastern Cree, adj.-verbs *kinwoosu, methosu, awkoosu, máskówissu*. In (Baraga's) Chippeway, *gin-osi* 'he is tall,' 1st p. *nin gin-ós* 'I am tall' ; *akosi* 'he is sick,' 1st p. *nind akos* ; *mashkawisi* 'he is strong,' 1st p. *nin mashkawis* ; *nókisi* 'he is weak, soft' ; 1st p. *nin nokis*.

* See *Trans. Am. Philol. Association*, 1869-70, p. 115.

Mass. *Ohtau* 'he possesses, *has*,' and intrans, *ohtaeu*, *ohteau* (El.), *ohtu* (Cotton), 'it *has itself*, it *is*,'—is used often by Eliot, to supply the place of the verb 'to be' when the subject is *inanimate*; e. g. *moskeht . . . uttiyeu yeu kesukok* OHTEAU 'the grass which to-day is,' Matt. 6. 30; *pish ohteau keyausut* 'it shall *be* in your flesh,' Gen. 17. 13: *nuppawonk ohteau ohkuhqut* "there *is* death in the pot," 2 K. 4. 40; etc. Baraga treats the corresponding verb, in Chippeway, *até* 'there is of it, it is,' as "unipersonal" (3d pers. sing. and pl.), and Lacombe gives it, in Cree, an *animate* form, "*ittaw*, il existe, il est," while Howse regards it as "the relative form of the verb-substantive *i-ow* [*ai-iu*, p. 163, ante], having for its attribute the relative prefix of place, *it*"; giving, with anim. subject, "*it ow* 'he is there' (Fr. *il y est*), and inan. *it-akwun* 'it is there.'"

The Noun, a Verbal. Putting aside all speculation concerning the priority of noun and verb and the relative prominence of the conceptions of objective and predicative relation, in primitive Algonkin speech, and looking only to the structure of the language at its present stage of growth, I repeat, that Algonkin nouns, so far as their derivations can be traced, are formed *from verbs*, or *as verbals* on predicative roots. Every name manifests consciousness of subjective relation—affirming that the person or thing *is*, or *moves*, or *acts*, or is *acted upon* by another animate or inanimate agent. This is seen to be true even in names which must have been of very early origin and which certainly were in use before the separation of existing dialects. Take the following examples:

1. RIVER. Mass. *sip-u*, *sip*, Abn. *sipu*, Chip. *sibi*, *sipi*, etc. From the root *sip* 'stretching,' 'extending,' hence 'to be long.' In the Massachusetts dialect, *sipi* (*sepe*, Eliot) is used adverbially for 'long' or 'extended' in *time* or *place*: e. g. *sepe-pomantam* 'he long lives,' 'is long-lived,' "stricken in years," Gen. 24. 1; *sepe kodtantum-up* 'he had long desired' &c. Luke 23. 8; *sepohtae* "long continuing," Jer. 30. 23. Lacombe (Diction. Crise) gives as the meaning of the root, "that which draws itself out, extends, enlarges itself; which

lasts a long time," etc., and for the verb *sipiw* "it extends, draws out," but he has not observed that *sipiy* (*seepee*, Howse) 'river,' is from the same root and a form of the same verb. In Chippeway, a difference of pronunciation distinguishes—and partly conceals the radical identity of—*sibi* 'river' and *jiba* "durable, lasting, extending." The former is in Chippeway, as *sipu* is in Massachusetts, a predicative form: 'it stretches, extends, streams.' Compare Chip. *jibi* "he stretches himself"; *jib-issin* "he lies stretched out" (Mass. *sipsin*); *jib-an* "it is tough" i. e. stretches. From the same root are formed Mass. *sipagham* 'he sails,' i. e. goes by stretching a sail; *sipagke-og* 'they are spread, spread themselves'; and *sipaghunk* 'a sail,' lit. 'that which is extended.'

2. FATHER. Mass. *n'osh* 'my father,' Narrag. *n'osh*, Chip. *n'oss*, Cree *n'otawi*: literally, 'I come from him' or 'he froms me' (giving the preposition the office of a verb transitive): in the 3d person (obviative), Mass. *osh-oh*, Chip. *oss-an*, Cree *otdwi-a* (*ottawiya*, Lac.), 'his father' = 'he comes from him,' 'he froms him.' The root *ū* (Eliot's *ω*) means 'going out' or 'proceeding from.' It takes verb form in

Mass. *n'oom* 'I come (proceed) from,' 3d p. *oom*, *woom* (El.)

Del. *n'ūm*, " " " " *wūm* (Zeisb.)

Abn. *n'oom-en* 'I came thence.'

With the vowel-change, in the subjunctive, Mass. *wag*, Abn. *wek*, 'he when coming (or, who comes) from' a place other than that of the speaker.

When the idea of motion or of animate energy is associated, *ω* becomes *ōch*, *ōsh*, or *ōtche*, and these, in some dialects, by nasalization of the vowel, have passed into (Del.) *ūntch*, or (Chip.) *ūndj*. Thus, with inan. subject,

Mass. *waban ootsh-oh* "the wind bloweth" (comes from) &c. John 3. 8.
(subj.) *toh wādsh-ont* "whence it [may] blow."

Del. *ta ūndch-en* ? "whence blows [the wind]?"

With an animate subject,—

Mass. *n'ooch-ai* 'I come from, am of' (a place): 3d p. *wutchau*.

n'oochai wohkumaieu "I am (come) from above," John 8. 23: *n'ooch-i Jehovah* "I am the Lord's" (from or of him), Is. 41, 5: suppos. *wudchiit*, *wachiit*, 'he, proceeding from,' 'he who is from.'

Chip. *nind ôndji* 'I come from': suppos. and particp. *wendjid*.

E. Cree, 3d p. *ooch-tü* (*ooche-oo*, Howse,) "from-eth he," "he proceeds from."

With the transitive form,—

Mass. *ooche-un* 'he produces from it': e. g. *ne muhpeteôg. . . ooche-un mittamwoos-sissôh* "that rib. . . made he [from it] a woman." Gen. 2. 22: with inan. obj. direct and remote, *ooch teau-un* 'he made of it,' Ex. 38. 8.

W. Cree, *osi-hew, oji-hew*, 'he makes (produces) him.'
osi-taw, oji haw, 'he makes (produces) it.'

Chip. *nind oji-a* "I make" some anim. object: 3d p. *od-ôji-an*.

nind oji-ton "I make" it. " *od-ôjilon*.

nind ojig "I am made" (produced), " *ojig-i*.

Impersonally — serving as preposition and as adverb — 'it is from, of, out of, because of,' and 'for that cause,' 'thence,' 'that from,' etc.,—

Mass.	<i>ooch-i</i> ,	with vowel change,	<i>wûlchi</i> ,
Abnaki,	<i>oots-i</i> ,		<i>wetsi</i> ,
Cree,	<i>ootchi</i> ,		<i>wetchi</i> ,
Chip.	<i>ondji</i> ,		<i>wendji</i> ,
Del.	<i>ûntchi</i> ,		<i>wentchi</i> .

From this verbal-particle—which receives conjugation forms — comes the name for 'father,' Mass. *n'ôsh*, Del. *n'ôch*, &c., meaning 'I am from him' or 'he produces me'; 3d p. *ôsh-oh* 'his father' = 'he is from him.' (Compare Mass. *waban ôtshoh* 'the wind comes from.')

3. MANIT, i. e. the *Preternatural*; often translated by 'God,' 'Spirit,' and the like, is regularly formed as the suppositive (3d pers. sing.) and participial of a primary verb meaning 'to surpass, exceed, be more than.' The verbal root, AN 'surpassing, going beyond,' is the base of Mass. *anuï* (*anue*, El.) 'it surpasses, is more than'; Del. *aluï* (*eluwi* "most," Zeisb.), West Cree *a'iu* (*Vayiw*, more, surpassing, etc., Lac.), Chip. *ani-* ("in composition, marks 'going on'"; Baraga). Eliot employs *anuï* to form the comparative degree, as in Matt. 18. 8, 9, *anuï wunnegen* "it is better." It takes personal forms in Mass. *an-in* 'it is rotten,' 'decayed,' i. e. goes beyond, is more than ripe, mature, or fit for use; and, with anim. subj. *aninnu* 'he is corrupt, rotten' (John 11. 39, Ps. 14. 1); inan. ptepl. *ne aneük* 'that which is corrupt' (Mal. 1. 14, Prov. 12. 4); anim. *noh anit* 'he who exceeds, surpasses, is more than' — the natural, common, or normal:

with the indefinite and impersonal pronominal prefix — *m'anit* 'somebody or something that exceeds, is *preternatural*. With the predicative affix, *manit-u* (*manitto*, *manittw*, El.) 'he, or, it *is* *manit*': *nen manitto* "I am God." Is. 43. 12. "They cry out *manittóo*, that is, 'it is a God,'"— says Roger Williams—"at the apprehension of any excellency in men, women, birds," &c.; and so "they say of *every thing which they cannot comprehend*." In composition, the prefixed *m* is not retained: e. g. Del. *Get-annito* (Zeish.) = Mass. *keht-annit*, 'the greatest *manit*' (for "the LORD God," Gen. 24. 7); Narr. *Kesukquánd* (= *kesukqui-anit*) "the Sun god," *Wompanánd* (*wompan-anit*) "the Eastern god," &c. (R. Williams).

The impersonal prefix, *m*, is similarly employed—for forming an appellative from a verb or verbal root—in Mass. *may* 'a path,' literally, 'a going-to' (*aditus*), from *a u* 'he goes to,' suppos. *e-i.*; *minni*, contr. *min*, 'a berry,' or other small fruit, literally 'a growth' or 'what is grown'; *maskeht* 'grass,' form *askeht-u* 'it is green,' i. e. not yet mature or full grown; etc.

Names of actions and of instruments are formed from the third person singular of the indicative present. If the verb is transitive, the 'nomen actionis'—corresponding nearly to our participial infinitive—has both *active* and *passive* forms: Ex., Chip. *dibaamdge* 'he pays'; *dibaamage-win* 'paying,' payment, *given* to another; *dibaamdgy-owin* 'being paid,' payment *received*: *ságyiwe* 'he loves'; *ságyiwewin* 'a loving' (love *given*); *ságyigosiwin* 'a being-loved' (love *received*); and from other conjugation-forms of the same verb come *ságaadisiwin* 'self loving' and *ságyidiwin* 'mutual loving'; *pakitéige* 'he strikes,' *pakitéigewin* 'striking,' a beating *given*, *pakitéigowin* 'being struck,' a beating *received*. More than twenty other forms of verbal nouns are noticed by Baraga (*Otch. Gram.* 29–32) and Lacombe (*Gram. Crise*, 19–25). In each of these forms, we find a distinct recognition of the relation of the activity to the *subject*—animate or inanimate. For example, *pakitéigan* 'a hammer' (striking instrument), from *pakitéige* 'he strikes,' is formally distinguished as the instrument with which an

animate subject strikes an *inanimate* object: *tchigatáigan* 'a broom,' from *tchigatáige* 'he sweeps,' is 'that with which *he* (some *person*) sweeps it,' etc.

Professor Fr. Müller* selects, "as an example of the Algonkin *nomen actionis*, [Nipissing-] Algonkin *pimosé*, Cree *pemootan*, to walk (gehen)." As a verb, *pimosé* 'he walks' has, in the indicative present, the forms—

Nipis. Alg.	Chip.	Mohegan.
Sing. 1 <i>ni-pimosé</i>	<i>nin-pimossé</i>	<i>n'pumseh</i>
2 <i>ki-pimosé</i>	<i>ki-pimossé</i>	<i>k'pumseh</i>
3 <i>pimosé</i>	<i>pimossé</i>	<i>pumissoo.</i>

Professor Müller maintains that these are forms of a noun with possessive affixes—not true *verb*-forms,—and that they mean, respectively, not 'I walk, thou walkest, he walks,' but 'my walking, thy walking, (his) walking.' We find, however, that all Algonkin dialects have true "nouns of action" or participial infinitives formed from the indicative present of their verbs, and have also nouns denoting the (habitual or conceived) actor or agent, formed from the suppositive mode of the verb, with a change in the vowel of the root. Thus, from Chip. *pimossé* 'he walks,' are regularly formed *pimossewin* 'walking, a walk' and *pēmossed* (Moh. *paumseet*, Edw.) 'one who walks, a walker.' If we examine more closely the form *pimossé* (Moh. *pumissoo*, Abn. *pemussé*, Mass. *pomushau*) we find it to be that of an animate active-intransitive verb, composed of the primary verb *üssu*, predicating *animate energy* (see page 164, ante) and a verbal root (Chip. and Cree *pim*, Mass. *pōm*, *pūm*,) meaning 'passing' 'going by,' found as a prefix to a great many derivative verbs, in all dialects. The literal meaning of *pimossé* is 'he passes *actively*' or by the exertion of vital energy.

Cree *pemootayoo* (*pimuttew*, Lacombe) has a different formation, *-üttew*, in composition, representing the verb *ituttew* 'he goes' (on foot, or by walking): e. g. *nestuttew* 'he is, tired of walking,' *kākāyāhuttew* 'he walks strongly, or briskly,' *asettew* 'he walks behind,' *newokatettew* 'he walks on all fours' (lit. 'on four legs'), etc. To Cree *-uttew*

* *Grammat. Bau der Algonkin-Sprachen*, 146.

corresponds Chip. *-ossé* (= *ussu* 'agit') as the base of verbs of walking: e. g. *mitossé* 'he walks (indef.)', goes on foot,' *babámossé* 'he walks about,' or, from one place to another, *ajeossé* 'he walks backwards,' *minossé* 'he walks well,' etc. The Algonkin name for the *foot*, Mass. *-üssit*, Chip. *-sid* (*o-sid* 'his foot'), Cree *-sit*,—never found without a pronominal prefix—comes from *ussu*, or from the verbal root *us* 'agere.' The foot is 'the doer' or 'the worker.'

In *pimosé* we have (1) the verbal root *us* 'agere,' (2) the *subjective* pronominal element *e* (Mass. *u*) expressing the predicative function, and (3) the associated root, *pim*, designating the mode of activity—in other words, qualifying the primary verb of animate activity.

To discuss the nature and origin of the so-called "transitions"—by means of which not only the *subject* but the *object*, animate or inanimate, direct and remote, may be incorporated with the verb—would extend this paper beyond reasonable limits. I have purposely omitted reference to these more complicated conjugation-forms, and have considered only such as may be assumed to be of earlier origin—older at least than the stage at which the tendency to extreme polysynthesis attained its largest expression. In the analysis of these, I can discover nothing that suggests doubt of their *genuineness*. The facts of language are seemingly opposed to the conclusion at which Professors Steinthal and Fr. Müller have arrived *a priori*. *Seemingly* opposed, I say, because I am not unmindful of Professor Steinthal's warning—that "some languages know how to supply the want of true form by devices so artful as *completely to attain the appearance* of real grammatical forms." (*Charakteristik*, 326.) The evidence may be summed up, very briefly:—

1. Algonkin verbal roots correspond, in general character, to Indo-European. As examples, take the following, found, with more or less phonetic modification, in all Algonkin dialects: *ap* 'sitting,' 'staying,' *pi* 'approaching,' *au* 'going to,' *ω* 'coming from,' *nad* 'bringing,' 'bearing,' *sīp* 'stretching,' *sok*, *sag* 'out-going,' *an* 'going beyond, exceeding,' *wab* 'seeing,' *pāk* 'opening,' *bok* 'breaking,' *pim* 'passing by,' 'crossing'; etc.

2. The predicative function is distinctively expressed, by the combination of a pronominal or demonstrative element with a verbal root.

3. The pronouns prefixed to the first and second persons singular of the indicative, are *subjective*, not possessive. In the subjunctive and imperative the distinction of grammatical persons is marked by *inflection* — not by pronominal affixes.

4. The distinction between noun and verb is formally established; though, as the noun is always derived from a verb or formed on a verbal root, it retains nearer likeness to the verb than in Indo-European languages. The distinction may be occasionally obscured by the subordination of the *noun* — in common with all other parts of speech — to the verb, but not by the prominence of the conception of objective relation, or by the want of formal expression of the predicative function of the verb.

5. The “*nomen actionis*” (corresponding to the participial infinitive) and the “*nomen agentis*,” are formed from the verbal root — the former being distinguished from conjugation-forms by its termination and the latter being characterized by a *change of the vowel of the root*.

6. Modal and temporal distinctions are marked in all conjugations: the former by changes of termination and by internal modification of the root; the latter by tense-particles prefixed to the verb (see page 160, ante,) and, for one tense — corresponding nearly to our continuous preterit — by a special termination (*-p* or *-b*, with euphonic connective; Ottawa, *-ba*; Chip. and Cree, *-ban*;) which seems to have come from root *ap* ‘sitting, staying:’ e. g. Mass. *n’t ap* ‘I sit,’ *n’t ap-ip* ‘I sat, or used to sit.’ This termination is given to *nouns* as well as to verbs: Chip. *n’oss* ‘my father,’ *n’ossiban* ‘my father who was, my deceased father;’ *nind akik* ‘my kettle,’ *nind akikoban* ‘the kettle which was mine.’

IX.—On a supposed Mutation between *L* and *U*.

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It is often stated that in French an 'u' has been derived from a Latin *l*, of which supposed examples are cited in 'sâlsus' French *sauce*; 'fâlsus' *faux, fausse*; 'bâlsamu^m' *baume*, in which the Latin *â* merely closes to *o*, independently of *l*, as shown in cases where the *l* and orthographic 'u' occur together, as in 'sâl-ix' *saule*; 'sâltus' old Fr. *sault*, modern *saut*. Similarly, Latin *o* closes to French *ou*, as in 'molînus' *moulin*; 'môrîri' *mourir*; 'môvêre' *mouvoir*; or Latin *u* remains as French *ou*, as in 'fulgur' *foudre*. The facts remain the same whether the 'au' is regarded as ô, or as representing a diphthongal sound in some of the dialects.

In some localities the 'e' of *beau* is pronounced, leaving *au* as a recession of *u* in 'bêllus,' influenced by the ablative form 'bello,' like the ô of English *cōulter* from Lat. 'cûlter' (coultter, knife). We have a like case in Latin -u^m, ablative -o, in going from 'mantelu^m' to Fr. *mante-au*; and final -a of 'bêtula' (birch) gives ô in French *b..ouleau*, Lat. 'et' being dropt and 'u' (ou) retained. So 'cûltêllus' gives French *cou..te..au* (= coo-to); 'aqva' *eau*; 'alter' *autre*; 'altus' *haut*; 'astur' *au..tour*, where Latin *s* is lost, *a* becomes *o* (or perhaps a diphthong in some dialects), and *u* (= oo) remains in its usual French orthographi.

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